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ART. I.—*Injuries of the Superior Extremities.* By W. L. SUTTON, M. D., Georgetown, Ky.

CASE I. *Gunshot wound of the axilla.*—A negro boy aged about 12 years received, March 11, 1842, the load of a shot gun in the right axilla, which passed out just behind the spine of the scapula, about two inches from the glenoid cavity. There was said to have been considerable hemorrhage at first; but when Dr. Smith saw him, in perhaps half an hour, there was none of moment—no pulse in the arm, which was ascribed to the shock. It does not appear that the left arm was examined. I saw him on the 12th, when he complained but little; no pulse at the wrist, (but in the left arm, it was distinct and irritable,) sensation imperfect, but not destroyed; temperature not materially diminished, if at all; tongue furred, whitish. Gave a dose of calomel. Reapplied a bandage on the forearm and

arm, and applied a bandage and compress in such a way as to act on the subclavian artery, below the humeral end of the clavicle. As he was five miles from town, and it was deemed important that he should be where he could receive strict attention, I directed him to be removed to Lexington, the place of his master's residence. I saw him no more, but was informed that he bore the transportation well. In a day or two mortification of the hand and fore-arm commenced. A well-marked separation between the dead and living parts took place above the middle of the arm; and about the 28th the arm was amputated at the line of separation. At this time, the original wound was suppurating favorably, and confident expectations of a favorable result were entertained. On the day succeeding the operation, he was seized with "spasms" (tetanus, I suppose), which recurred frequently until the 11th of April, when he died.

I should not have noticed this case, except to bring one fact to the notice of the younger members of the profession—a fact which I doubt not has been taught, but which I have no recollection of having seen or heard inculcated; which others *may* therefore be as little prepared to anticipate as I was. It is, that a sudden obliteration of the principal artery of a limb is not *necessarily* followed by a marked diminution of temperature in that limb. In support of this proposition, the only two cases bearing upon it, which have fallen under my observation, may be adduced. I refer to the foregoing case, in which I am satisfied that the axillary artery was either divided, or had its vitality destroyed by the load of shot; and to a case in which I tied the femoral artery about two inches below Poupart's ligament, in which neither the hand nor thermometer indicated an abatement of temperature.

Case II. *Arm torn off—injury to abdomen—death on the 11th day.*—Andrew, a negro aged 14, Dec. 16th, 1834, got his left arm entangled in the band, by which part of the machinery of a paper mill is propelled; he was drawn some ten feet to a partition wall, through which the band runs, with his abdomen directly against the end of a large beam, which ex-

tended through the wall, whereby his arm was torn off with scapula attached. I saw him probably in less than an hour—no pulse, surface cold; prescribed stimuli internally, and heat externally. In three or four hours the circulation improved considerably, when there being no hemorrhage, I dressed the wound by drawing the skin together as well as I could; but a small portion of the wound remained uncovered. Lint and adhesive strips completed the application to the wound itself; but it was judged proper to put a compress over the subclavian artery, to guard against hemorrhage, and another over the cavity made by the abstraction of the scapula, which were secured by a bandage, something in the manner of Desault's second for fractured clavicle.

19*th.*—Pulse has remained at 130; tongue acuminated, furred, white; skin dry; urine high colored, depositing a copious brick colored sediment; no appetite; complains little. To day he complains of tenderness in the abdominal muscles; and now for the first time I became aware of the fact before mentioned, that he was drawn with his belly against the end of the beam.

24*th.*—During the 19th he was troubled with bloody, slimy stools, which were attended with much griping. They continued three or four days, occasionally alternating with serous offensive stools. Much pain in the bowels occasionally, particularly at night. Pulse has been down to 104, but at present 112; tongue casting off fur, but has a rather dry and unpleasant appearance. The wound, where not covered by the skin, looks too dark; some imperfectly formed pus.

26*th.*—The serous, offensive stools continued. Occasionally he has a very severe pain in his bowels, lasting from 15 to 30 seconds. Considerable quantity of pus, of the color and consistence of cream, about the clavicle, and in the cavity from which the scapula was drawn. Over the clavicle the skin has sloughed, say two inches by three fourths, showing the bone dead. Pulse 124, small; tongue pale; sight failing. Died on the morning of the 27th.

Inspection six hours after death. Abdomen but moderately

distended. Upon opening it, the small intestines were seen of a mahogany color, with here and there a spot darker than usual. They were usually of the diameter of one and a half inches; but where darker, they were also of greater diameter, and had very distinct arborizations of bloodvessels. In numerous places, although the intestine was round before it was touched, yet upon being touched, it pitted, and did not resume the cylindrical shape. The large intestines were in general healthy; but the rectum, near its commencement, was contracted for three or four inches to less than half its usual diameter. Throughout the extent of the colon there were spots which presented the same dark color and arborizations of vessels as were observed in the small intestines. At these places also the gut was irregularly pouched. The mucous coat of the small intestines, and of the spots mentioned in the large ones, was of a rather dark mahogany color. The best idea which I can give, is to refer to Horner's Pathological Anatomy, plate 1, fig. 6, and say that the color was pretty uniform, not so dark as the deepest parts of the figure, and darker than the remainder; and to pl. 1, fig. 2, accompanying Jackson's paper on Cholera, American Journal of Medical Science, vol. 12, and say that the horizontal stripe near the middle is near the color, but not deep enough. The stomach externally appeared healthy, but was not opened. The liver healthy; gall bladder filled with healthy bile. Although the mesentery did not appear unusually dry either to the sight or touch, yet it crepitated like a dry membrane on passing a scalpel through it.

The wound of the shoulder had sloughed a good deal since the last dressing. The clavicle was entirely denuded of the periosteum for three or four inches from the humeral extremity, and two thirds of its circumference. The stitches by which the skin had been drawn together, had nearly sloughed out, although they shewed no such disposition at the last dressing. The cavity over the front of the chest, and that from which the scapula was drawn, shewed no disposition to diminish. The muscles and cellular membrane beneath the clavicle were

a good deal ecchymosed. The subclavian artery was filled with a coagulum from its origin to the point of laceration.

Did this boy die of the injuries done to the shoulder, or did he die of that done to the abdomen? I think the latter. He never complained of the shoulder, and it was only in the last days of his life, that sloughing shewed itself in the wound of the shoulder, whilst from the third day after the accident, there were considerable pain and great disorder in the bowels. It may be said that this disorder of the bowels supervened as a consequence of the injury done to the arm. Whilst I admit that such things do sometimes take place, I do not think that this was one of the cases. Every one will draw his own conclusions.

Case III. *Compound fracture of the humerus.*—W. K., aged 8, December 15th, 1830, fell from a horse, receiving the weight of his body principally upon the left hand. I saw him the next day. I found the humerus broken immediately above the condyles, the upper fragment forced through the skin on the fore part of the arm, to the extent of an inch and a half, which "two of the strongest men in the neighborhood had tried in vain to draw into place." The bone appearing healthy, a slight incision of the skin enabled me to reduce it. It was then put up in angular splints, and treated as directed by teachers. Nothing of note occurred, and in due time he was dismissed with a good arm.

Case IV. *Humerus fractured by a musket ball—artificial joint.*—Samuel Shannon, in the North West campaign in 1813, had a musket ball to pass through the humerus, immediately above the condyles. It would appear that no very strict treatment was pursued. After some weeks, he regularly bent his arm every day. His arm got well with the elbow stiff, and an artificial joint at the place of fracture. This artificial joint supplies the place both of the elbow joint, and of the rotatory motion of the fore-arm, in a very perfect manner, and he is able to do a good day's work at any kind of labor. I mention the case of this man, with whom I have lately become acquainted, to show what nature will sometimes do for

a man, and that an artificial joint is not *always* such a bad thing.

Case V. *Compound dislocation of the elbow—mortification—amputation—recovery.*—William Johnson, aged 13, fell from a height on his hands, Sept. 15th, 1839, by which the left elbow was dislocated, and the condyles of the humerus forced through the skin in front of the elbow joint. It was reduced and dressed in the straight position by Dr. Price.

16*th*. I saw him by invitation: much fever; pulse 134; tongue very much furred: fore-arm and hand cold, insensible to pricking, and perhaps a shade too dark; the capillary circulation not utterly destroyed, as would appear by pressing the skin, producing a white spot, which slowly disappeared.

17*th*.—Much the same, except that he was considerably delirious all night, and still continues so. Mortification has commenced upon the fore-arm. On consultation it was determined that amputation was the only chance of safety, which was immediately performed. He was in great danger for several days, notwithstanding amendment was perceptible in a few hours after the operation. After amputation, it was ascertained that the humeral artery, the two cutaneous nerves, and the veins in front of the elbow were torn asunder by the condyles of the humerus in their exit.

Case VI. *Dislocation of the elbow—sloughing of the fore-arm—artificial joint.*—Feb. 22, 1842, I was requested to visit in consultation W. C., aged 8, when I received the following history of the case. He fell from a fence some time in December, receiving his weight on his right hand, whereby the elbow was dislocated, the condyles of the humerus being thrown forward on the fore-arm. The dislocation was reduced and a bandage and splints applied in the bent position. Upon dressing a week after, sloughing of the fore-arm had commenced, which eventually carried away all the soft parts on the outside, back, and inside of the fore-arm, nearly to the wrist, leaving the bones naked. The wrist and hand swelled very much, and mortification of the thumb and one of the fingers ensued. At present the hand and fingers are enormously

swelled, but of a color suitable to that state; sensation is distinct. Necrosis of a large portion of the ulna has taken place, which portion has been removed; what remains is covered with red granulations. The radius gave way about the middle, yesterday. For about two and a half inches below the elbow joint, the soft parts remain sound; the elbow-joint flexible. He is represented as having remained in good health from the time of the accident; no fever or other disturbance. He is now in good health. The appearance of the arm presents to me the impression that the constitution is making a very considerable recuperative effort; and there being no circumstance to require immediate removal, it is agreed to wait on the efforts of nature. The arm was directed to be kept still, and in a case, so as to prevent motion of the fractured part.

June 14th.—I saw him accidentally; his arm in a sling without any other support; but it is said that he usually carries it in a box. The swelling of the hand has disappeared. There are two or three fistulous sores about the hand and fore-arm, and occasionally a scale of bone is discharged. The forearm is otherwise skinned over, and of good appearance; one of the branches of the radial artery has become the principal trunk. The ulna and radius have not united, but his father thinks the arm is more stiff. The elbow joint is not so flexible as it was in February. The fore-arm has shortened about two inches. No command of the fingers. The boy remains in good health.

It is too soon to say what will be the result of this case. At present the probability is that the ulna and radius will remain ununited, and that from disuse the elbow may become stiff. Yet it is possible that by proper attention both might be avoided. I certainly think that due care to prevent motion of the bones has not been used; but we dare not say that any care would have procured union.

Case VII. *Fracture of the humerus—compound fracture of the fore-arm—laceration of the external interosseal artery—arm saved.* Feb. 20, 1836, Peter, a very stout, athletic negro about 30 years old, in calendering bagging, got his hand caught between the cloth and spindle. In an instant the arm was wound round

the spindle to the axilla, and his body thrown over the spindle upon the cloth, which stopped the machine.

I found him soon after in great pain; the convexity of the back of the hand destroyed, but I did not ascertain any fracture there; the bones of the fore-arm broken in three places, at the lowest of which both had lacerated the skin; the radius still projecting; no portion of bone loose; the external interosseal artery torn and bleeding; the humerus fractured at the middle. The artery was tied, and the arm and fore-arm dressed in the usual way. Three or four hours after, he was in so much pain that it was thought advisable to remove the dressings and reapply them: now he also complained of severe pain in the chest, occasioned, I suppose, by the pressure of the body against the spindle, in being thrown over it. He was now bled, and had a decided dose of opium with calomel. He was not able to lie down for a week, and continued to suffer greatly. During that time, he took several cathartics (mercurial and mixed) and such other medicines as were thought necessary to keep down inflammation, with generally an opiate at night.

Dr. Craig and myself attended this case jointly after the first dressing, and thought it necessary to dress the fore-arm very frequently. In about five months it was well (several pieces of bone having been removed in the mean time), with no deformity in the arm; the elbow joint perfect; some deformity in the fore-arm, occasioned by a neglect or inability to keep the fragments of the radius duly separated from those of the ulna: a very imperfect rotatory motion of the fore-arm; an inability to shut the hand firmly, but with sufficient use of it to enable him to do a full day's work at spinning.

Case VIII. *Fracture of the humerus—fracture of the fore-arm —mortification—amputation—recovery.*—Peyton, a young negro man, Feb. 7, 1840, got his hand caught in calendering bagging. His arm was wound up like Peter's, with two fractures of the bones of the fore-arm and one of the humerus; no laceration of the integuments. The fractures were reduced and dressed in the usual way by Drs. Craig and Price. Three days afterwards, I saw him in consultation. Mortification had

commenced on the fore-arm, and was spreading rapidly up the arm; pulse about 100, not of a bad character; countenance not bad, but marked with suffering. Amputation was decided on and immediately performed. Mortification attacked the stump, but by judicious treatment was arrested, having made little progress.

Here are two cases very similar, the graver doing comparatively well, the less severe requiring amputation. Whence this difference in the result? From treatment? Not so; for I doubt not Peyton was treated as well as Peter. The accident found Peyton's system, constitution, body, or whatever term you choose to employ, less able to resist injury. Now as perhaps a majority of physicians upon seeing Peter's arm, might have thought amputation justifiable (if indeed not called for), and as few would have thought of amputating in Peyton's case, we are admonished not to have too little confidence in the powers of nature to resist injuries; at the same time to be prompt to interfere where circumstances require us to do so.

Case IX. *Fracture of the ulna—dislocation of the superior end of the radius—deformity.*—Aug. 10th, 1841, W. M., aged 10, presented himself, and I received this statement. Three weeks ago he was sitting on a fence, to which a horse was fastened, the horse becoming frightened, pulled the fence down with him on it, and hurt his arm; no distinct idea could be gathered as to the position in which he fell. Next day, when the arm was very much swelled, he was taken to a neighboring physician, who dressed the arm. Upon examination, I find the ulna has been broken at about one third its length from the elbow, and has united at a considerable angle, which, assisted perhaps by a softening of the ends of the fragments in the process of healing, has admitted a shortening of near an inch (measuring from the olecranon to the styloid process of the ulna), as compared with the other fore-arm. The superior end of the radius is thrown out on the outside of the tubercle of the humerus. For a moment I endeavored to force the button-like extremity of the radius into the lunated notch of the ulna; but a little reflection shewed me, that to effect that object, it would be necessary to lengthen the ulna, for which,

again, it would be necessary to break up the union of that bone. Although neither flexion nor extension of the fore-arm could be carried to the maximum, and the rotatory motion of the hand was imperfect,, yet as the motion of the elbow joint was very considerable, without pain or inconvenience, and as some rotatory motion existed; and as some uncertainty existed as to the state of the semilunar notch and the success of reducing the head of the radius (even if the ulna were broken again), and as there was danger of an artificial joint, I decided against interference.

Was this the proper course to pursue? As I am not aware that just such a case has ever been reported, I may be allowed to offer such remarks as present themselves to my mind. Dislocation of the radius is confessedly rare. I have seen, so far as I now recollect, the details of but two cases—one by Dr. Smith, of the Pennsylvania Hospital, Med. Ex., vol. 1, p. 197, and one by Dr. Hammond, Med. Chir. Rev., vol. 32, p. 285. In both of these the luxation was backwards. So far as I know, but one other kind of luxation of this bone has been recognised, to wit, forwards. A question then may be fairly entertained, was I correct in my opinion of the nature of the accident? The very remarkable prominence projecting off from the external protuberance of the humerus, as observed by sight and touch, and the distinct concavity in the end of the bone, as very evident to the touch, left no room for doubt. But, it may be said, there was too free motion in the elbow joint. There certainly was more than I should have expected; but it must be recollected, that this state of things had lasted three weeks, and the parts concerned had made some progress in accommodating themselves to their situation. Could the radius have been reduced without breaking up the attachment of the ulna? To have enabled the glenoid cavity of the radius to come into its proper position, the ulna remaining shortened, it would have been requisite to force the radial portion of the wrist joint at least three quarters of an inch beyond the ulnar portion. If this could have been effected it would have thrown the hand towards the ulnar edge of the fore-arm, beyond any allowable position. But say that a por-

tion of the excess of length should remain at the upper extremity. Then as soon as it was brought before the tubercle of the humerus, the motion of the elbow joint would be destroyed; or the outer condyle of the humerus forced backward, which, if it did not destroy the motion of the joint, would greatly injure it. Should the bone have been broken? Perhaps it would have been proper. But I repeat, that I do not know whether the cavity of the semilunar notch would remain three weeks in a condition fit to receive the head of the radius, or whether it would be found more or less filled with cellular substance, and of course presenting more or less difficulty in retaining the radius there when reduced.* In a recent case, what would be the best mode of proceeding? Any one perhaps could answer this as well as I could, but it appears to me, that extension and counter extension, with some force applied to the button-like end of the radius, assisted by rotation and straightening of the fore-arm, would be proper, and could scarcely fail.

Case X. *Fracture of the scaphoid cavity of the radius—fracture of the lower end of the ulna.*—Mrs. P., Septh 4th, 1838, was thrown from her carriage, and received her weight on the left hand and fore-arm, by which the scaphoid cavity of the radius was split, and the lower end of the ulna fractured just above the styloid process. I dressed the fracture in the usual way, and by attention, I had as much gratification as the most unqualified satisfaction expressed by the lady and her friends could give; yet I could see, that although there was no absolute deformity, still there was a want of symmetry, which I was much mortified to see in a lady's arm, especially as it occurred under my own management.

Case XI.—Nov. 1839, A. G., a carpenter, middle aged, fell about eight feet upon his hands, splitting the glenoid cavity of the left radius, and of course having a dislocation of the wrist. Being much dissatisfied with the usual mode of dressing in

* Mr. Liston, speaking of a simple luxation of the elbow joint, says: "It is almost impossible to replace the bones after three or four weeks; indeed, I have been foiled at the end of two weeks."—*Elements of Surgery, p.* 500.

such cases, and having seen a statement of Dr. J. R. Barton's mode of treating them, I determined to try it. Having the arm properly extended, I applied a roller upon the hand and fore-arm, securing a compress some third of an inch thick, and two and a half square, on the carpus and back of the hand, and a similar one on the palmar side of the radius, just above its carpal extremity, in such a way that the superior extremity of one corresponded with the inferior edge of the other. Over these I applied two thin but firm wooden splints, long enough to extend from the elbow to the points of the fingers. After a week the dressings were removed and reapplied daily pressing the fractured portion of the radius firmly so as to prevent displacement, whilst the fingers and wrist were fixed and straightened; and after ten or twelve days adding pronation and supination to these motions. At the end of treatment the wrist was found perfectly symmetrical. I hold myself under much obligation to Dr. Barton for instruction on this point.

ART. II.—*Miscellaneous Cases.* By SAMUEL HOGG, M. D., late of Tennessee.

[The following cases are taken from the Diary of Dr. Hogg, where they were recorded at various and distant periods, and probably without the most remote view to publication. They have nothing novel or particularly striking to recommend them, but they will be read with satisfaction by the physician engaged in practice. Y.]

J. M., aged 35 years, was taken on the 19th of July, 1814, with what he supposed to be bilious colic, having been subject to that complaint in former years. He had taken a cathartic when I visited him on the 22d, and was complaining of considerable nausea and burning heat in the stomach, fever, and frequent griping; his eyes and skin were yellow, his stools grey, and his urine high colored. I gave an emetic, which operated several times, but without any appearance of bile, and then moved his bowels, dislodging matter slightly bilious in its character. In the morning of the day on which I visited him,

he had had a chill. He was ordered to take 5 grs. cal. every three hours until his bowels were acted upon, and on the return of fever next day to lose 8 ounces of blood.

23*d*. Calomel had not acted. Directed cal. & jal. aa. 15 grs. to be taken immediately, and to be repeated in six hours, if his bowels were not freely moved.

24*th*. Had a chill in the morning; powders had operated three times, bringing off dark green passages; fever high, and sense of burning in stomach persistent. Bled him to the extent of 10 ounces; the serum of the blood was in excess, and appeared largely mixed with bile, the crassamentum coagulating slowly. Ordered cal. in 8 gr. doses to be taken every six hours.

25*th*. The cal. had not operated. Chill repeated; burning heat in the epigastrium continues. He lost blood again, and took sulph. sod., which acted on his bowels, inducing dark, fetid evacuations. Ordered blister to the region of the liver.

26*th*. His chill had returned; pulse still active; blister very much inflamed; general yellowness increased. He lost half a pint of blood, which appeared to consist chiefly of bile and serum. Bowels have not been moved by the last calomel; nausea distressing. Left him 5 pills of cal. of 6 grs. each, to be taken once in six hours.

27*th*. Had taken four pills, which had produced some small passages of dark green matter. Directed him to take the remaining two.

28*th*. Saw him a few minutes after he had expired. An hour or two before he died, he vomited a considerable quantity of a dark colored matter mixed with blood.

Case of Milk-sickness.—On Tuesday night, 14th Dec. 1830, I was called to see Thomas Hunt, 30 years of age, who informed me that he lived on Goose Creek, in Sumner County, and that some of his family had recently been affected with what was called milk-sickness, in consequence, it was supposed, of their having used the milk of cows laboring under the trembles. He left home for Nashville on Thursday, the 9th, and was seized, after much fatigue, with symptoms of the same disease. The symptoms were, pain in the head, chilliness, nau-

sea, irritability of stomach and vomiting. When I saw him five days after he was attacked, his bowels had not been evacuated; sense of burning in the stomach and bowels insupportable; vomiting on taking any thing into the stomach, the matter ejected being of a greenish color, and of a peculiar odor, which he said was an invariable attendant of milk-sickness. His tongue moist, clean, smooth and of natural color; general surface dry, cool, and rather livid, features shrunk, and indicating extreme distress; pulse 110 in the minute, and laboring.

Treatment. Venesection was attempted, but the sinking of his system compelled me soon to close the orifice. Ol. ricina. with 40 gt. tinct. opi. was administered, but not long retained. Fomentations of the abdomen and extremities were adopted, and pills of cal., rhu. & al. were directed in doses to move his bowels. Next morning he had taken four pills, but without the desired effect. Symptoms more alarming; patient becoming comatose, but when roused gives correct answers; great thirst; says his bowels are burning up; passes no urine. Enemata being used, he passed some very hard, dark fecal matter and bloody sanies. Cal. 30 grs. & tinct. opi. 40 grs. were administered and retained. Sinapisms were applied to the stomach and extremities. At 12 o'clock, M., 10 drops croton oil were given, and at 2 P. M. the dose was repeated without effect. Difficulty of swallowing is becoming great, pulse sinking, great chilliness. 4 o'clock. Patient is evidently sinking. Takes strong tea of rad. serpent. with paregoric as long as able to swallow. Expired about 12 at night.

Cases in Midwifery. Mrs. M., a lady of delicate habit, was taken with the usual signs of approaching labor on a Thursday morning; the midwife waited on her till Friday evening, and labor then not advancing, I was called in. I found her with considerably increased arterial action, the os tincæ but little dilated, and the child apparently large. She lost at once a pint of blood, and took 40 drops of laudanum. Her pains came on regularly, which had not previously been the case, but the os tincæ continued rigid, and the child advanced very slowly. In about five hours she was again bled to the extent of three gills, and the opiate was repeated.

The os tincæ in about three hours now began to dilate, and the membranes being found on examination to be protruded, were immediately ruptured and the waters passed off. I now found the head, which was very large, just entering the passage, and rather confined between the sacrum and pubis. Her pains began to decline, and as she was much exhausted by the labor and the officiousness of the midwife, I concluded to allow her an opportunity of taking some rest, and accordingly ordered her to be put to bed and not disturbed till necessity should compel her to rise. She slept an hour, and on waking the pains returned with great force, and in about an hour she was safely delivered of a very large boy. She had borne three children previously, with two of whom she was obliged to have assistance. The blood drawn showed on cooling considerable buffiness.

A negress of stout frame and good health was seized with labor pains on a Monday morning, having given birth to one child before. Her pains were irregular and not very strong, but in the evening, the midwife being anxious to deliver, ruptured the membranes with her finger, whereupon the pains ceased, and I was called in. I found her resting quietly, though much fatigued, and complaining of soreness, the result of excessive handling; the os uteri was rigidly contracted, and the head barely in reach of the finger. Her pulse being active, I took a pint of blood, gave her an opiate, and directed that she should be allowed to rest. She slept about three hours, and on awaking she had some pain, but as they were inefficient, I again abstracted blood from her arm. I left her at 10 o'clock at night, and charged the attendants not to interrupt her if she were inclined to sleep. In the morning I was summoned in haste by the midwife, who insisted on my opening the head of the fœtus, as she concluded it to be dead, and thought the woman could not be delivered without it. On examination I found that the head had advanced a little, but the os uteri was still rigid, and the strength of the patient being unimpaired, I drew blood a third time. In half an hour she had a call to the close stool, where she was seized with a severe pain, and on examining her soon afterwards, the head

was found nearly cleared, and the next pain expelled the child alive. I then waited a few minutes, when the placenta, after a slight pain, was safely expelled, and the woman afterwards did well.

Mrs. J. S. A., a woman of robust constitution and mother of six children, was taken on a Friday with the usual signs of approaching labor. When the midwife was sent for about 4 o'clock in the morning, the labor was well advanced, and about 11 o'clock, A. M., she was delivered of a boy. Immediately after the birth of the child the midwife proceeded to clear her, although she appeared still to be unusually large; but the placenta being firmly fixed the cord was ruptured in the effort to force it away. In the course of the next hour the midwife made frequent attempts to get it away, but failing, she became alarmed and sent for me. When I arrived I found the woman quite easy, but much alarmed with the idea of a false conception, which I soon dissipated by an examination which disclosed the presence of another child. As it was high in the pelvis I abstracted a little blood, and giving a few drops of laudanum, exhorted her to patience. In an hour, having a call to the urinal, she distinctly perceived the motion of the child. Finding that she was in no danger, the old woman having fortunately failed to dislodge any portion of the placenta, I suffered her to rest until sunset, some six hours after the birth of the first child, when, her pulse being active and the friends pressing that something should be done to expedite delivery, I took a little more blood. Next morning about 10 o'clock her pains began to return, but were weak. I gave her 40 drops of laudanum, with a tea-spoonful of tinct. camphor, which had the effect of increasing the pains, and at 1 o'clock the head was found far advanced. The child, a second boy, was born in half an hour afterwards, but was saved with difficulty in consequence of the cord having been wound twice around its neck so as nearly to stop the circulation. In a few minutes a single placenta was expelled without hemorrhage, notwithstanding that the cord had been ruptured and the umbilical vessels open for twenty-seven hours. The woman did well.

REVIEWS.

ART. III.—*Researches in Embryology, in Three Series.* By MARTIN BARRY, M. D., F. R. S. E., Fellow of the Royal College of Physicians in Edinburgh.

These researches, though *three* in *series*, are *one* in *character* and design, constitute but a single Memoir, and are the product, almost entirely, of the scalpel and the microscope. The immediate subject of them, though not altogether new, is rare and curious. No American writer has ever seriously handled it—at least not extensively; nor, as far as we are informed, has any British one, except Dr. Barry, and the late Mr. Cruikshank.

To the physicians of continental Europe alone, especially to those of Germany and Prussia, and more recently to a few French physiologists, does the theme appear to be in any measure familiar. And even they, until the present century was somewhat advanced, had, in no small degree, neglected it, for considerably more than a hundred years—ever indeed since the time of De Graaf. But the study of it is not only revived; it is becoming active and even fashionable. Nor will the physiologists of England be indifferent to the example. For, throughout the civilized world, fashion controls science almost as much as dress. And, as an embryologist, Martin Barry, though comparatively but a young man, is already conspicuous.

Having qualified himself for the task, through reading and travel, by the acquisition of a thorough and correct acquaintance with all that had been previously written and known,

respecting embryology, that ardent and enterprising student of nature entered on the further investigation of it, by analyses and observations of his own, with a degree of industry and zeal, energy and perseverance, that have rarely been equalled, and never perhaps surpassed. And the memoir he has produced sufficiently corresponds with the time expended and the skill and dexterity bestowed on it. For its length, it is one of the most elaborate essays in the English tongue—or indeed in any tongue with which we are familiar. And if it cannot be called a learned production, the reason is plain, and much more creditable to the author, than any amount of *borrowed knowledge* could make it, though dignified by the authoritative title of *erudition*. It occupies some ground previously unexplored, and therefore new, undescribed, and unrecorded. Yet it shows that, as relates to the subject of it, Dr. Barry's literary researches have been as extensive as they could be. For, as already intimated, that indefatigable inquirer has left, as we believe, no work unexamined, in any language, that could be made available in preparing him for his enterprise, or that could aid him in the prosecution of it. He has, therefore exhibited, in his investigations of this curious branch of science, all the learning attained or attainable, either by himself or any other person; because he has exhibited all that is extant.

But however interesting and valuable these "Researches" may be to the reader who attentively studies and thoroughly masters them, they constitute but an indifferent subject for a review. The reason is plain. They are already so dense a mass of facts, that it is scarcely possible to render them more dense. They cannot, we mean, by being thrown into the form of a summary, be reduced to so narrow a compass, as to be presented in substance to the reader at a single view. And were we to attempt to give him a competent acquaintance with the memoir, and thus do justice to its author by extracts from it, our review would be lengthened far beyond the usual extent of the articles admitted into this journal. The subject, moreover, is so intricate, and, to be fairly mastered and under-

stood, requires such closeness and fixity of attention, that, curious and valuable in physiology as it is, we doubt whether it would be prized by a majority of our readers in proportion to its merit.

All we shall attempt therefore at present will be, to submit to the reader a succinct representation of the object aimed at, by Dr. Barry, and of the course pursued by him, in his attempt to accomplish it. This being done, we shall offer a few remarks on a point or two of doctrine embraced in his memoir, but not, in our opinion, correctly expounded. In this discussion we may probably offer some thoughts of our own on a topic connected with the subject of his researches, but not introduced into them. We allude to the process of menstruation.

The scheme of our author in his "Researches" is of a nature not easily defined, or characterised by words. Though strictly *elementary*, it is neither actually original, nor marked by any real profoundness of thought. Yet, pointing as it does to the very incipiency of things, it includes in it a spirit of enterprise so bold and aspiring, as to be hardly unworthy of the epithet sublime. This is the more especially true, from the consideration that its aim is to throw light on a subject which was once deemed inscrutable. And the difficulty of the project may be almost said to have justified, at the time, an entire disbelief in its practicability. But time and means, judgment, resolution and patient labor are all but omnipotent, in the affairs of man. There are few *earthly* ends that they will not attain, if, under such regulation and direction, perseverance be pushed to the requisite extent. Of this truth Martin Barry stands forth himself an eminent example.

The Doctor was not merely content to *begin his labors*, where preceding inquirers into embryology had *closed theirs*. That step, so decidedly unambitious, would have deprived him entirely of all bold and lofty pretension to originality, which he was unwilling to resign, and would have rendered him, by his own acknowledgment, a subordinate writer. He therefore as-

cended, or at least endeavored to ascend, as his point of departure, to the very fountain-head of organic existence.

Commencing with the real seminal or primitive animal matter, even before it had become organized, he has traced, with uncommon minuteness and care, and also with great apparent accuracy, its progress through all its subsequent changes of size and figure, and in all its multiplied and diversified elements, until the complete formation of the ovum, its entrance into and progress through the fallopian tubes, and its lodgment in the uterus. And there is good reason to believe, that, in his close and keen examination of these mutations and movements, he has not only made several interesting discoveries of his own, but also corrected certain errors contained in the writings of preceding inquirers, and thus doubly contributed to the advancement of embryology.

His observations, it is to be understood, were made almost exclusively on the mammiferous class of animals. And he believes (we doubt not correctly) that he has settled the question (if indeed it were not previously settled) as respects the place both of the *formation* and the *impregnation*, of the ovum, in that class. The ovary he considers the site of both processes. And that those processes occur in the same place in all viviparous, and perhaps in all oviparous animals also, is hardly to be questioned.

In alleging that Dr. Barry has "*settled*" the question respecting the site of the formation and impregnation of the ovum, we do not claim for him any thing new. We attribute to him only the full and satisfactory confirmation of the opin ion of another. By those who have paid attention to the subject it is known, that upwards of a century and a half ago, De Graaf pronounced the ovary to be the seat of both these processes. The opinion, however, was subsequently opposed by Haller and his followers, who contended that the fallopian tubes were the places where the ovum was formed out of matter derived from the ovaries, and deposited in the tubes in a shapeless and immature condition; and that impregnation was effected in the same organs. In this opinion we say Dr. Barry

not only concurs with De Graaf, but has established its truth.

For the benefit of those who may wish to repeat his experiments, it is necessary to mention, that our author conducted his researches chiefly by the dissection and inspection of female rabbits, during the period of their rut or sexual desire, especially just before and after the act of copulation. And he was so fortunate as to collect evidence still farther confirmatory of the fact, well known before, that the rutting or amative season of the mammaliæ, is uniformly accompanied by an augmented vascularity of the ovaries, and other parts of the generative system, as well as by a preternatural accumulation of arterial blood in them. This phenomenon in the females of the inferior animals, as will hereafter be made more fully to appear, is analogous to menstruation in woman, and is no doubt designed to subserve the same purpose—the preparation of the generative organs for the process of impregnation, and, in woman, for the continuance of them in that condition.

As a farther element of the history of the memoir we are examining, it may be mentioned that it first appeared as a communication to the Royal Society of London, and was printed originally, and we believe exclusively, in the Transactions of that distinguished body. The plates contained in it are very numerous, of great diversity, and highly finished. They are also of great value, as without the demonstrations they afford, the text of the work would be unintelligible. Such, as we are told, was the estimation set on the memoir, by those appointed to judge of its merit, that it opened to its author the door of the Royal Society, of which he is now a member. So much for all the general remarks we deem it requisite to make on the memoir.

We have alluded to a point of doctrine maintained by our author, in the soundness of which we do not concur with him, and which we have promised to make the theme of a brief discussion. It is contained in the following extract from the memoir.

"As to the particular *locality* in which the ovum becomes

susceptible of development [*is impregnated*] physiologists are not agreed. Some maintain that it acquires this susceptibility before it leaves the ovary; others that the change is not effected until after its expulsion from that organ.

"It is not my intention te discuss the question whether contact of the seminal fluid with the ovum is or is not essential to impregnation. Yet perhaps it may be proper to refer to the *possibility of that contact*, while the ovum is still in the ovary, that having been denied."

To demonstrate this possibility, and we may say, to show his *belief* in the *actual occurrence* of the contact which he pronounces *possible* (for such belief he virtually avows) our author continues.

"In *seventeen* out of *nineteen* instances of the rabbit, though the parts were generally examined while warm [*immediately* after the death of the animal—and also soon after coition] I was unable to discover spermatozoa in the fluid collected from the surface of the ovary. In the other *two* instances, however, spermatozoa, or at least animalcules exactly like those I had been accustomed to meet in the uterus and vagina, were really found in the ovary."

That the ovum is impregnated as well as formed in the ovary, is a point of doctrine, in the truth of which we have long believed, and have often defended it, for the most part *orally*, but also through the press. The striking changes which take place soon after coition, in one or more of the ovarian vesicles, previously to any discharge from them, furnish abundant evidence of the fact. But that the semen masculinum, whose influence is essential to impregnation, makes its way to the ovary, and, in its formal and entire character, comes into immediate contact with the ovum, or containing vesicle, we deem not only doubtful, but altogether incredible. And though we do not pronounce the event *impossible*, we regard its improbability as of the highest order—of the same order indeed with that of the spontaneous *ascent* through the air of a mass of iron, or the *descent* of a volume of hydrogen gas

without any compulsive force, the atmosphere being tranquil, and in a natural condition.

The cause of our disbelief, moreover, seems to us reasonable and plain. We consider every positive and well-established fact of the case as testifying strongly against the hypothesis. Nor, of course, if this be true, is there, or can there be a law of animal action in favor of it.

Some of the facts alluded to shall be presently cited. Previously however to this, we shall specify the principal modes, by which the semen masculinum is supposed to be brought into contact with the ovarium, and state our objections to them.

1. Some physiologists (perhaps a majority of those of the present day) contend that that fluid is introduced into the uterus, during coition, *vi jaculationis penis masculini;* and that thence, by some *secret* spring of movement (for none such has yet been discovered, nor scarcely even fancied) it glides or hurries forward, enters and passes along the fallopian tubes, issues out of them again, and finally reaches the surface of the ovarium. Nor is its journey yet terminated. It penetrates the ovum, and comes into immediate contact with what our author terms the "germinal vesicle;" and there the work of impregnation is completed.

Such is the darksome journey—half by compulsion—half spontaneous—supposed to be performed by the semen masculinum, in quest of the place of action which awaits it. And that it is an arduous one, the most sanguine of its advocates must acknowledge. To us the series of obstacles opposed to it, even when the generative organs are in a sound and perfect condition, appears to be insuperable. And under the influence of certain derangements of them, which not unfrequently occur, without preventing the process of impregnation, the impediments to the passage of the fluid toward the ovarium are so much augmented, that, in her efforts either to remove or surmount them, even fancy herself, from a lack of expedients, must falter and fail. Of the difficulties which thus present

themselves, when the generative organs are in a healthy condition, the following are a few.

The axes of the vagina and the uterus do not so correspond in direction, as to meet and constitute a straight line. At their junction with each other they form an angle, which, though far from being acute, effectually prevents the semen masculinum from being projected into the *os tincæ*, and through the neck and strait, into the cavity of the uterus. The semen, when ejected in coition, may strike the uterine cervical extremity; but that it cannot enter into and pass through it, every fact bearing on the subject conclusively shows.

In the virgin state of the female generative organs, the rugæ formed by the lining of the vagina constitute another obstacle to the projection of the semen. Nor is this all. The viscosity and weight of that fluid, united to the adhesiveness of the mucus spread over the lining of the whole canal forms a further very serious obstacle to its projectility through a crooked and very narrow opening, or rather through no actual opening at all (for the sides of the canal are in contact) into the cavity of the uterus. In truth it appears to us, that a more impracticable and hopeless project can hardly be imagined than that of introducing the seminal fluid, during coition, *vi jaculationis*, into the uterus.

But admit the difficulties of the introduction to be vanquished, very little is yet done toward the completion of the enterprise. At least the most arduous portion of it remains to be performed. The fluid has lost the whole impetus or force of movement received during coition from the male organ. Nor does there exist, in the surrounding parts, a single organ or portion of machinery, to communicate to it a fresh impetus. It must first, by some instinct and locomotive power of its own, grope its way to the fundus uteri, there search out the scarcely more than microscopic orifices of the fallopian tubes, enter them, and then crawl along those lightless canals, until it shall finally arrive at the ovarium, there to aid in the perpetuation of our race. We say *our;* for our observations relate to hu-

man impregnation. Yet are they applicable to that function on a much broader scale—probably to *universality itself.*

But, according to the tenets of the hypothesis we are examining, the travels of the semen masculinum are not at an end, even when it has perched on the ovarian vesicle which contains the ovum. It must pierce through the parietal membrane of the vesicle, as well as through the proper membrane of the ovum, before it can operate effectively on the "germinal vesicle" of that body. And, if we mistake not the hypothesis, on that it must operate, else impregnation does not follow. *The contact must be complete.*

That the discussion may be conducted, on our part, with the utmost fairness and liberality, we again admit what we consider inadmissible, because we conscientiously believe it can never occur. We concede that the seminal fluid has not only reached the fundus uteri, but that it has made its way into the minute orifices of the fallopian tubes. But how is it now to continue its journey, and finally reach its place of destination? The tubes can by no possibility conduct it there. They are in the condition of a broken-down bridge, which positively refuses to the traveller a passage. To drop figurative language, and express ourselves in that of our profession, the fallopian tubes are in a *flaccid condition.* But when in that state, they do not, by a considerable distance, extend to the ovaries, and cannot therefore convey to them and deposit on them aught they may contain. To perform that office they must be in a state of *vital erection*, prepared for efficient functional action. For surely our opponents will not contend that the seminal fluid, by being merely admitted into the fallopian tubes, passes through them without their aid, and equally without their aid, makes its way to the ovary. Yet notions no less preposterous have had their advocates. Nor are we by any means confident that fancies as visionary, respecting impregnation, are not now entertained and defended by individuals who rank with the gravest and wisest of our philosophers.

Does any one ask us how we know that the fallopian tubes

are flaccid during sexual intercourse? To this our reply is prompt and positive. We know it because it has been ascertained by observation. And Dr. Haighton, one of the first physicians of his day, was the experimenter and observer. He killed a female rabbit, when in full rut, *immediately after coition*, and the fallopian tubes were completely flaccid. Nor did they exhibit the least sign of having been recently rigid, or injected with blood. He then proceeded to kill other female rabbits, also in rut, at every hour after coition, until the *ninth* hour, when incontestable signs of impregnation already presented themselves; and the fallopian tubes were but just beginning to prepare themselves to pass into a state of rigidity. They were beginning, we mean, to be preternaturally injected with arterial blood. And wherefore were they so? The cause may be easily rendered. Their action was to be soon required to receive, convey to the uterus, and there deposit, the contents of some ripe ovarian vesicles, which were preparing to burst, and commit to the fimbriated extremities of the tubes the matter within them. These experiments were several times repeated with the same results. Nor have either these, or the many other experiments of Dr. Haighton respecting impregnation, though cavilled at and inveighed against, ever been subverted or in the slightest degree invalidated by the experiments of any other writer on the subject. Indeed they cannot be subverted, for the best of reasons. They have been conceived with judgment—and accurately performed; and their foundation is truth.

To speak of this matter plainly and irrespectively, and apply to things the terms that truly expound and graphically characterize them. The circumstances of the case compel us to regard as essentially preposterous that hypothesis which contends that the semen masculinum passes into *positive contact* with the ovarium. Once more we emphatically ask, and solicit to the question the serious and deliberate attention of physiologists—by what means is the passage effected? or, with the present organization and endowment of the parts concerned, by what means can it be effected? And we challenge a

calm and philosophical reply—a reply abounding in fact, reason and argument, and free from conjecture, assertion, and fancy.

That by the projectile power of the male organ, in the act of coition, the seminal fluid can be thrown into and through the uterus and fallopian tubes, and thus be brought into actual contact with the ovarium, we again declare to be *physically impossible.* Admit, we repeat, that it can be thrown into the uterus, *vi jaculationis;* it can be thrown no further. As far as the original impetus is concerned, there it must stop. How then is it to be put in motion again, and conveyed to the ovarium, its supposed place of destination? The uterus most assuredly has no apparent fitness, in either structure or habit (as far as we are apprized of its habits), to conduct it thither, in a course of motion directly the reverse of that which its customary action produces. For it need hardly be remarked by us, that the usual, we believe the *universal* direction of the action of the uterus is from the fundus to the neck—not from the neck to the fundus. But did it convey the seminal fluid to the openings of the fallopian tubes, the latter must be the course of action taken on by it; because no other would produce the desired effect. Nor were the fluid actually introduced into the uterine end of those tubes, do they appear to be in the slightest degree qualified to convey it to the ovarium. Why? Because the action suited to such conveyance would, in its direction, be exactly the reverse of that which they are positively known to perform. Matter has been often found in them, in its passage from their fimbriated extremities to the uterus, but never journeying in the opposite direction. Never has any substance been detected in them *certainly* passing from their uterine to their ovarian ends. All allegations to that effect are but unsubstantial guess-work.

It is definitively known, we say, that the fallopian tubes carry matter from the ovarium to the uterus. Were it equally certain that, *when in a healthy condition,* those same tubes carry matter, by their *natural* action, in the opposite direction, the fact would constitute a phenomenon *unique,* as we

believe, in human physiology. We remember, at present, no other instance, in the system of man, where a tube, by its natural and healthy action, is alleged to convey matter, either solid or fluid, in opposite directions, at different times. The healthy functions of our systems are coincident and harmonious. If, in any case, contrariety or clashing occur in the economy of our bodies, it is the result of a condition of things neither sound nor natural. The mere fact that the cystic bile *twice* traverses the cystic duct—first toward the gall-bladder, and then toward the duodenum, can with no force or justice be cited in contradiction of the general truth in physiology here contended for.

Will any one allege that the spermatic animalcules—the *homunculi* of former writers, and the *spermatozoa* of those of the present day—will any one fancy or even dream that those microscopic specks of matter, containing within themselves the impregnating and quickening leaven of vitality and being, and possessed of muscular or some other sort of locomotive powers, crawl, and grope their way, from the vagina, or the neck of the uterus, through the worse than Cretan labyrinth of the generative apparatus, until, without either chart, compass, or clew, they arrive at the ovarium, enter into the ova, complete the work of impregnation, and, becoming themselves embryos first, and infants next, are ultimately developed and matured into adults? Will any one peril his reputation for intellect, by avowing himself the advocate of a notion like this? We think not. Yet would the fancy, wild and visionary as it is, be just as consistent with reason and common sense, as any of the elements of what may be called significantly enough, the CONTACT HYPOTHESIS—of that hypothesis we mean which, in its scheme of expounding things, attributes to *contact* an influence and a consequence far beyond what it possesses—which, in particular, doggedly maintains, that of bodies in contact the mutual action is not only much more certain, but also much more *explicable*, than of those that are not in contact—and which, in fact, virtually contends, that their mere *being in contact* is of itself a satisfactory reason *why* bodies

do act on each other, and their simply *being apart*, a sufficient season why they *do not*. But of this *contact-hypothesis* due notice will be taken hereafter; while, in the mean time, we shall proceed in the discussion of our more immediate subject.

The foregoing are some of the difficulties to be encountered by those who would conduct the semen masculinum to the ovarium, the genital organs of each sex being in a natural and healthy condition—such a condition, we mean, as offers no impediment to their most efficient action. And the difficulties of the case are greatly augmented, in magnitude at least, if not also in number, by various derangements which occur at times in the organs, while the power of propagation remains, *certainly undestroyed*, if not unimpeded. Of such derangements the following may be noticed.

The *projection* of the seminal fluid into the uterus is augmented in difficulty by the *truncation* or shortening of the penis, by the loss of a large portion of its entire body, as well as by strictures of the urethra, which scarcely allow even the urine to pass. And such *projection* is rendered impossible by anomalous openings of the urethra, not at the point of the glans, but on the underside of the penis, *behind the glans*. Yet, while laboring under these embarrassments, men have often had children born to them. Instances of the kind have fallen under our own notice, while, as related to the fidelity of the mothers, no shade of suspicion was entertained.

But the most hopeless obstacle to the passage of the semen masculinum to the ovarium remains to be mentioned. It is the entire occlusion of the vagina, the os tincæ, or the neck of the uterus. This impediment, absolute while it continues, is effected in several ways; by imperforated hymeneal membranes, which have unquestionably existed; by morbid adhesions, the product of inflammation, which have also existed; and by tumors arising from various causes.

That under the obstruction to the passage of the seminal fluid into the uterus, offered by these anomalies, impregnation has been effected, we are forbidden to doubt. Testimony to that

effect is transmitted to us, on authority as high and valid, as any which the annals of our profession afford. Among the individuals who have recorded themselves as witnesses of such occurrences may be mentioned Harvey, Morgagni, Hildanus, Ruysch, Simpson, and Haller—men whose veracity has never been impeached; whose competency to investigate and judge has never been questioned; and whose contributions to medicine have immortalized their names. Nor is this all. Several strongly attested cases, of a more recent date, are publicly recorded, and have been reported to *ourselves* in private letters from individuals both of intellectual distinction and high moral standing; and we have in person witnessed a case, on which we could put no other construction. But to close our descant on these difficulties, all of which are formidable, and some of them insuperable, and pass to another subject.

Perhaps there is no exercise of the mind in which a greater display of *false philosophy*, or as it might well be denominated *philosophical folly* is made, than in that of stubborn adhesion to what we have called the CONTACT HYPOTHESIS. And more or less abundantly that hypothesis throws its influence into every profession, and every department of human research.

The medical adherents to it, who constitute a large majority of the Faculty of christendom, can neither comprehend, nor apparently believe in the action of medicinal substances, except they be in some way, in their mode of employment, connected with *contact*. Hence, if such substances prove in any degree effective, they are believed to be dissolved or mixed in the blood, which conveys them directly to the deranged portions of the body, and thus enables them, through immediate contact, to act on and cure or relieve their disease.

For this high efficacy and influence of contact these gentlemen contend, without having, in many cases, the slightest evidence that such contact exists (for the medicinal substances which they believe to be mingled with the blood cannot be detected there), and without forming a *definitive*, or, even tolerably *exact* idea of the import of the term.

Strictly speaking we are hardly if at all authorized, *as philosophers*, to say that any bodies, of which we have definite knowledge *are in mutual and positive contact.* We can pronounce them only *near to each other.* Why? Because bodies of any appreciable size can, by certain powers and processes, be brought still nearer to each other, than they are in what is called a natural state. And the same is true of even the minute constitutional particles of matter existing in masses. They can be still more closely approximated to one another. In other words, each mass can itself be more consolidated, which implies an increased proximity of its particles. As relates to bodies not brittle, more especially if they be possessed of life, this is certainly the case. Their constituent molecules can be brought nearer to each other than they are in their usual condition. Of all masses of fluid matter, quicksilver itself not excepted, the same may be affirmed. By compression and otherwise the nearness of their elementary atoms to one another may be increased. A few further remarks on the term *contact* may aid us therefore in rendering our views, in the present instance, the better understood.

In preciseness of expression, contact is undefinable; because it has no definite and positive meaning. It is an abstract or general term bearing relation to space, and indicating degrees of proximity that may be pronounced almost innumerable, and therefore exceedingly different from each other under different circumstances, and in different cases. It might be called, with sufficient significancy, the PROTEUS of the English language. Were we to attempt to affix to it any settled definition, after having pronounced it "undefinable", we would say that it signifies (if indeed it can be made to signify any thing) the shortest space that does or can exist between two bodies—or the shortest space we can measure in fancy. Like the notion of the infinite divisibility of matter, it is an abstract truth, but a practical fallacy. On this abstruse and interesting point we shall only add, at present, that notwithstanding all that is said about contact and its effects, it is almost certain, that between solid and ponderable bodies, it *has no existence.* Such bodies appear to be kept

apart by a subtle, elastic and imponderable aura; which extends to all places, and penetrates all things, but of which nothing is known except from its effects. Might we coin a word suitable, as we think, to the present occasion, we would say, that, as relates to both massses and molecules of *ponderable* matter, this *imponderable aura* seems to act the part of a universal *betweenity*—a substance which lies between them, and so far separates them from each other, as to allow them room for some *sort of action*—for we cannot doubt that, more or less, *every atom of matter acts a part* in the scheme of creation. In a system marked by positive and *consummate wisdom and perfection* (and such is and *must* be the handiwork of an ALL-PERFECT BEING), there can be no *idleness*. For idleness is essentially *imperfection*.

If all this be true then (and the refutation of it is fearlessly challenged), how fallacious are the views, and how futile the labors of our *contact*-philosophers, who conceive themselves in possession of a talisman, competent to the solution of some of the most obstinate difficulties in physical science! And that talisman is *contact*. And that contact is nothing more than an *imaginary or supposititious* relation. Yet do those mistaken inquirers seem really to believe, that if they can bring two bodies together, and place them in what they call *contact*, they fully understand and can clearly expound the reason and mode of their operating on each other; and that if the bodies be *not* thus brought together, they cannot operate on each other at all.

Such we say appears to be the groundless notion of those votaries of contact, while it would be easy to show, had we leisure to dwell on the subject, that it is by the mutual influence of bodies *not in contact*, but remote from each other, that the economy of nature is chiefly carried on. In proof of this we might refer to the organization and movements of the heavenly bodies, which are maintained and regulated exclusively by *remote agency*. The agents in this magnificent and sublime department of creation, are termed gravitation and repulsion, or rather the principles of those high and important forms of action. And to the natural and effective operation of those principles, distance between the bodies acting on

each other is essential. Were the several orbs which belong to the solar system, and those still more remote from us in space brought into contact or close proximity to each other, there is reason to believe that the economy of creation would be disastrously changed; and that little short of general ruin would ensue, and a new order of things succeed to the wreck.

When we advert to terrestrial matters, and take a survey of them, phenomena similar in character, and governed by similar principles of action, every where present themselves to us. In confirmation of this we may again refer to gravitation and repulsion, in all their multifarious effects and bearings, and also to the workings and manifestations of magnetism, electricity and galvanism. In the production and play of the phenomena proceeding from these agents, bodies remote from each other are in all instances and necessarily concerned; and in none of them is contact either indispensable or useful. On the contrary, it would derange and confound the whole scheme of action, now so beautiful, harmonious, and perfect. And there is reason to believe, that the same all-pervading, subtle, and ever-active aura, to which reference has been already made, is essentially influential in the control of the pageant.

But we have not yet closed our remarks on the influence and operation of distant bodies on one another. Far from it. The most striking and appropriate to our present purpose are yet to come. And they will bear relation to living matter, especially to the human system, in the performance of its functions in both health and disease. The following are a few of the facts by which our position is sustained.

When wine or other cordial and exciting articles of food or drink are received into the stomach of persons exhausted by long fasting, fatigue, or otherwise, they throw their influence *instantaneously* throughout the entire systems of those who swallow them, and refresh and invigorate the most remote in common with the most proximate organs of their bodies. And this they do long before it is possible for them to reach

in substance, through the circulation of the blood, the portions of the system which they thus revive. Certain poisonous substances in like manner, if introduced into the stomach, or even applied to the lips, tongue, or eyes, act with equal suddennesss on the most distant parts of the body, and produce death. Nor is this all. Deleterious impressions made on the skin by cold, burning, or in any other way, derange by sympathy the internal or remote parts of the body as certainly, and almost as speedily and seriously, as if they were made on the sympathizing organs themselves. And irritation on the lower extremities is known to have produced gastric spasms, tetanus, mental derangement, and death. Innumerable other instances moreover of a like description might easily be adduced.

Actual contact then between bodies, whether it be positively producible or not, is not necessary to their acting on each other. As far as we are informed on the subject, this is true of the entire scheme of material creation. But it is preeminently so, as relates to the economy of man, and other forms of living matter, in which all action, whether natural or unnatural, healthy or morbid, is the product of *stimulation* —the product, we mean, of an influence wholly unknown to *dead matter*, and of which dead matter is unsusceptible—the product of an attribute peculiar to *living* matter, and which is as entire a stranger to chemistry, as chemistry is to mathematics or theology. And it is perfectly well known that for the production or change of action in a part, whatever may be its character, *remote stimulation* is as efficacious as *proximate*. Our meaning will be understood to be, that an organ may be, and often is, as fully and effectually excited and thrown into action, by suitable impression on a distant organ, as it can be by impression on a contiguous one, or even on itself.

On the authority of the foregoing facts, then (and many others we say of like import might be cited), we confidently repeat, that, to whatever extent appearances and even authorities may testify to the contrary, *contact* between bodies

is not indispensable to their highest and most efficient degree of action on each other. In many cases it is unfavorable to such action, and in not a few entirely preventive of it. In a special manner it furnishes no facility in detecting the causes of action; or in giving any correct philosophical exposition of it. It enables us to assign no reason, why, on scientific grounds, the action may not have as certainly occurred without contact as with it.

It has long been matter of surprise to us, especially as relates to living beings, that this truth, obvious and incontestible as it is, should be so completely overlooked. For overlooked it is by almost every one—the cultivated and enlightened portion of the community forming no exception to the oversight, any more than the uncultivated and ignorant.

The universal impression is, that, provided an agent, whether salutary or deleterious, be brought into contact with any organ or part of the body, all difficulty, as respects causation is removed, and the reason why such agent produces a given effect on the part it touches completely understood. And *mere contact* is the talisman honestly though heedlessly believed to solve the difficulty, and render obvious to every one the entire cause of the action produced, and the result that follows. Yet to the philosophical inquirer and the clear and able thinker a moment's serious and strict examination cannot fail to render it certain, that the simple contact of bodies does no more toward the disclosure of the cause why they act on each other, nor toward the exposition of the mode of their action, than would their remoteness from one another do, or than would any other practicable posture, bearing, or condition of them do. For we repeat what we have heretofore stated in substance, that as regards a fitness to disclose the cause of the action of one body on another, *contact* and *remoteness* are in a like degree *nullities*. It is the *properties* of bodies, not their *positions* or mere *exterior relations* that fit or unfit them to act on one another. So much for our general doctrine. Now for its application to the subject of our inquiry.

Notwithstanding the foregoing truths, which, though we

may *deem* them incontrovertible, we do not *dogmatically proclaim* them so, nor claim or even solicit for them aught save a deliberate and impartial examination—the stricter the better—notwithstanding these considerations, most physiologists seem to think, that if they can succeed in showing that the semen masculinum, positively finds its way to the ovarium and touches it, they have explained satisfactorily the process of impregnation. The mere coming together of the male and female products appears to be to them sufficient for the purpose. It would seem, in their estimation, to explain to them the mode of operation of the two substances on each other, and to reveal to them the causes of the events that succeed. So deep is the deception which these physiologists practise on themselves, as well as on the public, by the negligence, and superficialness of their mode of research! Those gentlemen moreover under the influence of a quixotic spirit, seem pleased with the pastime of creating or imagining difficulties, and placing them in their own way, that they may display their prowess and dexterity in attacking and vanquishing them. In the present case they have two difficulties—one of them *artificial*, being needlessly manufactured by themselves, and the other erected by the hand of nature. The first is the difficulty of conducting the semen masculinum to the ovarium; and the second, the difficulty of knowing how to dispose of it on its arrival, and to understand and expound the office it has to perform. Nor would the latter difficulty be at all diminished could they actually bring the seminal fluid to the ovarium, and diffuse it over the surface of that organ, or even introduce it into its interior. They could as easily discover and explain its mode of operating and producing impregnation, when at a distance, as when thus in contact with the ovarium, or even when within it. Their hypothesis is in no way or degree benefitted therefore, by the more than herculean task of conveying the seminal fluid to the female organ, where they are ambitious to place it.

We, on the contrary, content with encountering a single *natural* difficulty, create unnecessarily no *artificial* ones.

Hence we decline the needless, useless, and impracticable labor of conducting the semen masculinum to the ovarium. We have therefore to contend with only the simple natural difficulty of expounding the influence and operation, *by sympathy*, of the semen masculinum on the ovary and its contents. And that we make no effort to expound, convinced of the impracticability of the task. We only admit the process as a fact, and illustrate and sustain, as best we may, our views in relation to it, by means of analogy. We repeat however that there is no more difficulty in accounting for the sympathetic influence of the semen masculinum on the ovarium, when deposited at a distance from it, than there would be, were it applied immediately to its surface—or even introduced into an ovum and brought into contact with the "germinal vesicle." And to make good their enterprise, we say again, the *contact-defenders* must thus introduce it, else their labor is lost, and their scheme defeated. Of the sympathetic theory of impregnation more will be said hereafter in detail.

Another class of physiologists equally bent on bringing into contact, in the work of impregnation, something of the masculine, and something of the feminine gender, have recourse to their fancy, and, by the creativeness of that, produce what they denominate a seminal aura. This they derive from the semen masculinum deposited in the vagina, and conduct it through some route (no matter what) to the ovarium, where, by contact with the ovum, or rather by making its way into it, it effects impregnation. But this aura exists only in the notions of visionaries. Indeed the whole hypothesis is but a mere creation of fancy, and shall therefore be dwelt on no longer, as a subject of discussion.

But we are not yet done with this phantom-tale of *contact*. A third set of physiological schemers, resolved on sustaining their notion, and making it subservient to a favorite hypothesis, convey in imagination the semen masculinum to the ovarium by a third and more circuitous channel. They contend that that fluid, being absorbed from the vagina, where it is deposited, *during coition*, enters into the blood vessels, mingles

with the blood, and, by that fluid, in the course of its circulation, is brought into immediate *contact* with every part of the body, and of course with the ovarium, where it effects impregnation.

To every one who may seriously reflect on it, the fallacy, not to say absurdity of this hypothesis, cannot fail, we think, to be obvious and glaring. Were it true, the case would be alarming to virtuous women; because the effect might be the peopling of the world with swarms of *illegitimates*. The entire mass of blood in the impregnated female would or at least *might* be imbued with the male seminal fluid, to such an extent, as to render it contagiously operative on other women as well as on its owner. In such a state of things, simply to introduce into the veins of an unimpregnated female a portion of the blood of a pregnant one, would produce in the former a condition similar to that of the latter. In plain terms it would impregnate her also. Thus might animal reproduction be promoted, like small-pox, or the vaccine disease, by simple inoculation. At least, ludicrous as the fancy may be thought, we perceive no good reason why the event might not occur, could the whole mass of a woman's blood be so saturated with semen masculinum, as to impregnate herself. But we must dwell no longer on so dreamy a conception. There exist many other hypotheses of impregnation, founded also on the notion of *contact*, all of which are untenable, and some of them, especially that of Buffon, wild even beyond the fictions of romance. In the citation and discussion of them therefore we must consume no more time.

Having thus avowed our dissatisfaction with all the *contact-hypotheses* on impregnation, were we asked, in what theory we are ourselves believers? we would reply; in that which *rests on the doctrine of sympathy*. But we shall speak of it cautiously. Our meaning therefore is, that we give a preference to that over all other doctrines, without positively averring it to be true, beyond contradiction, doubt, or cavil.

Of this theory the ablest and most laborious advocate and sustainer was Dr. Haighton. And to us his experiments on

the subject are as satisfactory and conclusive, as are those made by any inquirer, or on any physiological theme, that can be named.

It is a matter of regret to us that a want of room forbids us to submit a detailed account of those experiments to the scrutiny and judgment of our readers, and compels us to rest content with laying before them, in general terms, a brief exposition of the principles of the theory. And that this exposition may be the clearer and more definite in its elements, and the better understood, a few preliminary remarks will be offered.

That no mistake may exist as to the exact meaning of sympathy, as to be here interpreted, it is requisite for us to observe, that it is a term employed to denote the fact, that an impression made on one part of the human body, when possessed of life, produces excitement and action in another part, near or distant, as circumstances may direct. And the kind of action excited depends chiefly on two contingencies—the nature and character of the exciting body—and the organization and nervous endowment of the part that acts. Something also depends on the condition of the body as relates to health. In a particular manner the production of a specific effect requires the influence of a specific cause.

A well known fact in physiology, interesting in itself, and important to be borne in mind in the present discussion is, that sympathy in woman is much more vivid and powerful than in man, and therefore capable of the production of a deeper result. And another fact equally well known is, that the *genital* system of woman controls more powerfully her *general* system. To these facts are to be added another of equal certainty—that each portion of the female genital system sympathizes strongly with every other portion of it; and that it is these cords of mutual sympathy which *constitute it a system.* These positions are to be regarded as so many fundamental truths in physiology, and as such to be remembered in the sequel of this discussion.

Another important physiological truth to be also borne in mind, on the present occasion, is that the ovarium is essentially a glandular body, capable, like all other glands, of producing out of the blood something which the blood itself does not contain in a formal state. By means of this *creato-constructive* power (might we compound a term expressive of our own opinion) the ovary gives being to the ovum, in all its elements, which, *substantively* but not *formally*, had been previously in the blood.

A farther truth of interest and moment to us is, that, as already stated, vital action is the result of *stimulation*, and without stimulation of some sort could never be produced. The notion that it is or can be the result of any form of chemical agency, is as utterly absurd as would be the ascription of mechanical products to *moral means*.

Of stimulation it may be correctly enough said that there are two classes or kinds—*proximate* and *remote*. Of these the former arises from impressions made immediately on the stimulated part, while the latter is the product, *through sympathy*, of an impression made on a distant part. And in various instances, each sort of stimulation excites *secretory* and *nutritive* or *formative* action in perhaps an equal degree. As a general rule, however, there is reason to believe that remote stimulation is most efficient—ministers, we mean, to the performance and steady maintenance of the greatest number of important functions. This truth, too, our readers will do us the favor to bear in mind.

To the production of the ovum, both secretory and formative action are essential. By the former the requisite materials are prepared from the blood. By the latter those materials are constructed into the ovum. We shall only add, in this place, that secretory and formative action are excited by remote stimulation, as certainly and effectively as by any other kind—very often much more so. In the following instances the truth of this position abundantly appears.

1. *Secretion of tears.* This secretion is excited by those impressions on the brain (for they are nothing but forms of

cerebral excitement) called *joy* and *grief*. It is also excited by irritating impressions on the eye-balls and eye-lids—and in children by pain in any part of the body—in each case the lachrymal gland being at a distance from the stimulated point. Were the primary irritation made on the gland itself, there is no reason to believe that the result would be a secretion of tears.

2. *Secretion of saliva.* This is excited by impressions, whether agreeable or disagreeable, on the tongue, lips, and palate—as well as by the sight and odor of agreeable food, and the remembrance and conception of savory substances. Yet in neither case is the salivary gland proximately excited. The excitement it experiences is exclusively sympathetic.

3. *Perspiration or sweat.* This cutaneous discharge is excited by an impression on the stomach, produced by liquids either cold or hot. When a person heated by hot weather or severe exercise swallows hastily a draught of cold water, his skin secretes promptly an abundance of sweat. On the same principle of sympathy an immersion of the feet and ancles in warm water often excites perspiration over the whole body.

4. *Secretion of bile.* This secretion is excited through sympathy by an impression made on the mucous lining of the stomach and intestines, by calomel, aloes, and sundry other medicinal agents. Nor have we the slightest reason to believe, that an impression made immediately on the liver, by sprinkling the same articles over its surface, or injecting them into its blood vessels, would be equally effective in the production of bile. On the contrary, hepatitis, accompanied by a suppression of the biliary secretion, would much more probably be the result of such an experiment.

5. *Secretion of urine.* Of this secretion the same may be said. It is best promoted by remote stimulation. An impression made on the stomach by ripe water-melons produces frequently, and in a short time, a copious flow of liquid from the kidneys. The same effect is often induced by a draught of cold water alone—by water impregnated with some vegeta-

bic acid, or by the application of cold water to the surface of the body.

Of the secretion of semen masculinum we forbear to speak in detail, else could we easily show, that it also is essentially connected with remote stimulation. And of the pancreatic, the serous, and the mucous secretions, it would be easy to make it appear, that the same is true.

Exemplifications of formative or constructive action show themselves abundantly in the general *nutrition* of the body, and in all sorts of tumors, and other forms of unnatural growth.

The foregoing are the principal kinds of secretive and formative action in the body. And that they are all promoted, and may be greatly augmented, by remote stimulation, no physiologist will venture to deny. Nor can he refuse to acknowledge that to proximate or immediate stimulation their production is not necessarily, nor even usually attributable. And we fearlessly repeat, that were the stimulating agents, which give rise to the sympathetic action, applied to the secreting organs themselves, we are not authorized to believe that the healthy functions of those organs would be in any measure increased. The reverse, we doubt not, would certainly occur.

But why should we dwell any longer on this subject? The principle for which we are contending is a general one. Every vital action, of whatever form or description, we repeat again, is the result of stimulation. And a vast majority of these actions are of necessity the product of remote stimulation. Indeed the effects of this sort of stimulation are seen in all parts of the body; those of proximate or immediate in comparatively but few parts. And this is true, whether the system be in health or disease, as might be easily shown, had we time and room to prepare and exhibit an analysis of the subject. In the course of such analysis we could make it clearly appear, that the parts of the body are few in which it is even possible for proximate stimulation to occur.

Such are the remarks which we have deemed it necessary

to premise, that we may be the more easily and clearly understood, in the exhibition we are about to offer of the sympathetic theory of impregnation. And we trust they will prove sufficient for the purpose intended. As further preparatory however to their application, we ask the reader to bear in mind the well known and important fact, to which allusion has been already made, that every organ or part in the generative system of woman sympathizes very strongly with every other part. The commencement and course of the process of impregnation appear to be as follows.

I. The semen masculinum being deposited in the vagina, produces there its specific impression, which can alone be productive of the specific effect intended to be the second step in the process.

II. With this vaginal impression the ovarium next sympathizes, taking on a specific and corresponding mode of action. And that mode is such as, by its secretory and formative character, to give the complete and finishing touch to the ovum—a principal element of which is the "germinal vesicle."

III. The third form of action that shows itself sympathetically, is in the fallopian tubes, which by becoming rigid, and more highly charged with arterial blood, and of course with vital energy, and undergoing other changes in size and figure, pass into a state of preparation to receive the ovum from the ovary, and convey it into the uterus.

IV. Next occur, by sympathy, in the fundus uteri, the changes designed to fit it for the reception, attachment, and general accommodation of the ovum, by the formation of the maternal portion of the membranes, and the placenta. Nor is this all.

On the same principle, and in promotion of the same object, a change occurs in the action of the membrane which lines the neck of the uterus, the design of which is, the obstruction of the canal leading into the vagina, to prevent the escape of the ovum in that direction.

Nor do the changes connected with impregnation end here. Far from it. The entire female system undergoes a striking

change. The mammæ enlarge and soften, the nervous system becomes more sensitive and excitable, uncommon tastes and longings, sentiments and propensities often occur, and, toward the close of gestation, strongly-marked and important changes occur in the external organs of generation. These last changes are preparatory to parturition, which, without them could never be effected. But we must proceed no farther in detail on the subject.

The foregoing, though not all, are some of the leading changes, which, in consequence of impregnation, occur in the genital organs, and in other parts of the system of woman. And it is not easy to decide which of them is the most strikingly interesting and important. Yet the act of *immediate impregnation* excepted, they are all acknowledged to be the offspring of sympathy.

In truth to deny this, or even to question it, would be eminently preposterous. It would involve the weak and extraordinary notion, that the semen masculinum by its *actual presence in every altered part of the body, performs the whole work.* That it stimulates the fallopian tubes, increases their size, changes their shape, expands their fimbriæ, and renders them rigid. That, by its own direct and immediate action, it stimulates the fundus uteri into the preparation it makes to receive, and retain, and accommodate the ovum. That, on the same ground, and by its own immediate influence, it induces the mucous lining of the neck of the uterus to act in such a way as to close the mouth of that organ, the more certainly to prevent the loose ovum from escaping into the vagina. And that it extends in substance to the mammæ, and produces in them the changes they undergo.

All these changes we say must be produced by either the actual presence of the semen masculinum *in*, and its action immediately *on* the organs changed, or by *sympathy* with the primitive change produced by it, as most other persons allege, in the ovarium, but, as we believe, in the vagina, where *we know* that it is deposited. That any physiologist, we repeat, can even sportively imagine, much less seriously con-

tend for, the former of these modes of the production of such changes, we believe to be impossible. Some form of the latter therefore must necessarily be adopted, or the subject dismissed, as altogether inscrutable.

If then sympathy produces all the other changes which occur after impregnation, in various parts of the female system —in the fallopian tubes, the fundus uteri, the body and neck of the uterus, the mammæ, the external organs of generation, and elsewhere—if sympathy does all this, may it not also produce (what would seem to be much more easily done) that change, denominated impregnation, which occurs in a small portion of the ovarium? A reply to this question in the affirmative is but a simple inference from the *major* to the *minor* —a conclusion that, if sympathy can accomplish the former achievement, great and complicated as it is, *a fortiori* it can accomplish the latter.

We defy the ablest and most ingenious opponent of the sympathetic doctrine to render even a specious reason why, if for the production of impregnation, the *presence* of semen masculinum on the ovarium be *indispensable*, it is not equally so at the mammæ, and other parts of the female system, to produce in them the changes which follow. Nor will any one, on fair terms, accept the challenge, from a full conviction that the day will go against him. We, on the contrary, hold ourselves prepared to submit to our readers as plausible and sound reasons why semen masculinum should be present in the mammæ, to produce in them the secretion of milk, and the other changes in action and condition which they undergo during pregnancy, as the ablest humoralist can give, why medicinally substances of any description must be *formally* mingled with the blood, in order to prove either beneficial or prejudicial to deranged organs.

At the risk of being deemed unnecessarily wordy, we again express our regret, that a want of room prevents us from republishing Dr. Haighton's account of his experiments in defence of the sympathetic theory of impregnation. As far as we are informed, these experiments have never received from

any quarter, a reply of force sufficient to shake them in the slightest degree; and, as already intimated, they are, in our own opinion, conclusive of the controversy.

Were it admissible for us to protract this discussion any farther, we could also draw from the vegetable, and the lowest orders of the animal kingdom, sundry analogies highly favorable to the same theory. But we content ourselves, for the present, with a brief statement of the following curious and interesting fact. And for its authentiaity we refer to writers on entomology.

The aphis (cabbage-fly, or rather cabbage louse) produces in the course of the year several generations of descendants—one of them *oviparous*, and all the others *viviparous*.

The oviparous is hatched in the spring—say in April or May. This is followed suscessiyely by the viviparous, throughout the summer and early autumn, when, at a more advanced period, another generation, or deposition of eggs occurs, which lies dormant during the winter, and is hatched again in the following spring. And thus, from year to year, goes on their reproduction. But the most striking and extraordinary feature of the process remains to be mentioned, and is as follows.

The first spring-brood (that we mean which is of oviparous birth) consists of males and females. These unite and celebrate their loves, like other insects, and the males immediately die.

The next generation is viviparous, and all are females. This generation, without sexual intercourse (the males being all dead) gives birth to another generation, which, in like manner, consists also entirely of females. This race produces a a third, still viviparous, and still of the same sex. And thus viviparous reproduction proceeds, until some time in the autumn, when another crop of eggs is deposited to be awakened into active life, and moulded into due form, by the returning spring. Such are the facts of the case. Now for the philosophy.

Here are several successive generations (we think from five to seven) where the female has become pregnant, and given

birth to offspring, without copulation. Whence, we ask, is derived the male semen, to come into contact with the ovarium, as an indispensable event, and produce impregnation? To the advocates of the contact-hypothesis the question is addressed, for such solution as may await it, from their *wisdom* and *philosophy*.

We ourselves very frankly confess, that we have neither the one nor the other adapted to the subject. To us the event is therefore inexplicable; yet not more so than every other connected with generation. For the entire process is inexplicable. And in real inexplicability there are no degrees. The whole achievement is a dark mass. We understand no better how impregnation is effected with copulation, than without it. The impregnation of the female aphis after the death of the male, and therefore *without* his immediate influence, we understand just as well, as we do the impregnation of any other female animal *with* the influence of the male, and during his lifetime. Nor is there, as we confidently believe, any more necessity, as regards impregnation, for the semen masculinum to come into contact with the ovarium in woman, nor any more truth in the notion that it does so, than there is in the case of the female aphis, in those impregnations which occur in her, after the death of all the males of her race.

In no other case, except that of the female aphis, do we contend for the existence of *conclusive* evidence that impregnation can be and is effected, without the *possibility* of contact taking place between the male seminal fluid, and the ovarium. But in that case the impossibility is complete.

Analogy therefore, amounting to strong probability, which, under a deficiency of positive evidence, has great weight, forcibly indicates that a similar condition of things may prevail in the human subject—similar, we mean, as to the actual impossibility of contact occurring between the semen masculinum and the ovarium. And our reasons for believing that it positively does prevail, have been already submitted to the consideration of the reader. A few thoughts on the probable purposes and uses of *menstruation* shall close this article.

Though we believe it to be, with physiologists, the prevailing opinion, that the function of menstruation is peculiar to woman (the females of all the inferior animals being strictly *amenstruous*,) yet it is asserted by naturalists not unworthy of credit, whose opportunities to become acquainted with the matter in question have been favorable, that the case is otherwise; and that the females of the ape tribes are menstruant.

Another attribute deemed peculiar to woman is, that she admits, at all times the sexual embrace, the females of the inferior animals admitting it only periodically, and generally after long periods of intervening abstinence. But to this usage also the female ape is represented as an exception. She is said to receive the male, if not at *all* times, at least at much shorter intervals, than the females of other tribes of the inferior animals. Of these facts (for the truth of which however we do not fully and definitively vouch) the legitimate bearing will presently appear. But previously to this a remark or two on the nature of the menstrual function may not be amiss. And the object at which we immediately aim, at this moment, is sufficiently embraced in a correct reply to the following question.

Whether is menstruation a hemorrhagic, or a secretory function? in other and plainer words, whether is the menstrual fluid real blood, or something prepared from the blood, by a secretory process? To this interrogatory our reply is brief. We deem menstruation a secretory function, for the following reasons.

1. The menstrual fluid is not blood. Why? Because it does not, like blood, coagulate; nor does it, of course, contain any true fibrin, the master element, and the true cause of the coagulation of blood. It consists of only red globules, gelatine, and serum, which, of themselves do not, we repeat, constitute blood—wanting, as they do, the most important element in the composition of that fluid. In cases of real uterine hemorrhagy, the fluid discharged does coagulate, contains fibrin, and is therefore genuine blood. In evidence of its true coagulation,

it is discharged in clots. But the menstrual discharge, in a natural and healthy condition, is always fluid.

2. The odor of the menstrual fluid is peculiar—we might say specific—and altogether different from that of blood. It is truly as *sui generis*, as the odor of assafœtida, castor, or urine.

3. The menstrual is a *dead* liquid, blood a *living* one. When chemically analyzed, the former affords results quite different from those of the latter, under similar treatment.

4. Like other glandular bodies, the uterus receives more blood than is requisite for its nourishment. Hence like such bodies it frees itself from the superfluous portion of that fluid, not by a hemorrhage, but a secretory process. All natural and healthy discharges of fluids from the human body are the results of secretion.

Another question respecting menstruation is frequently asked, and differently answered by different physiologists. Whether is it a healthy and natural, or a morbid and unnatural process? Answer. The former certainly. It is as necessary to the health of woman, and therefore itself as healthful a process, as is that of digestion or the secretion of bile. Amenstruous women are never healthy. Is it possible then for deranged action in an important and influential organ to contribute to the sound condition of the whole body? An affirmative reply to this question from the lips or the pen of an enlightened physiologist would greatly surprise us. We ourselves reply, that we deem an issue of the kind physically *impossible;* because it is contrary to all we know of the laws and economy of nature. To contend or even admit, that an unsound *part* can tend to or effectuate the production of a sound *whole*, would amount to as positive an absurdity, as any other self-contradiction that can be uttered or conceived.

True; amenstruous women do occasionally bear children. But that avails not. They rarely if ever bear healthy children; and are *never* healthy during gestation. All observation compels us to believe that menstruation is essential to the perfectly sound and efficient condition of the generative or-

gans and their functions in woman. In fact the notion that the healthy propagation of our race depends on morbid action in the propagating organs is so utterly preposterous, as to be unworthy of a serious effort to refute it. It is so self contradictory as to set nature at war with herself for the purpose of insuring a salutary result—of actuully *bringing physical good out of physical evil.* As reasonably and well might the advocates of this hypothesis contend, that some morbid affection of his genital organs renders man also more competent to the propagation of his race, than he would be without it. Indeed reasons not easily refuted might be given, why, toward the production of a sound and vigorous offspring, the health and good condition of women should contribute even more than those of men. Corroborative, and even confirmatory of this, if we mistake not, is the result of observation in all enlightened nations, where the subject has received the attention it deserves. In Turkey, Persia, and Arabia, the belief in the doctrine here suggested is universal among the more intelligent and observing classes of society. The Arabs apply it in a particular manner to their domestic animals. Hence young and sound Arab mares of the *first order* are rarely if ever sold, or given as presents. Not one has ever been imported into America; and few if any into Europe. Males on the contrary are habitually sold and exported as presents.

As respects the design and usefulness of menstruation, some light may probably be thrown on them by a comparison of the process with an analogous event which occurs in the females of many sorts, perhaps indeed of all sorts of inferior animals of the higher orders. We refer to what is called the *rut* or *heat*, a condition of the genital organs which appears to be designed for two purposes—to awaken and vivify the sexual appetite in the female, so that she may freely admit the male; and to prepare her organs for the work of impregnation. We consider these to be the intention and use of the rut in the females of the inferior animals, because it is during the period of the existence of that state of the parts, and no other, that they admit the male to sexual connexion with them, or are susceptible of impregnation, were they to do so. And, as already

mentioned, the rut in the females of inferior animals is strongly analogous to menstruation in woman. So similar indeed are the two conditions to each other, that the mere *discharge* of the menstrual fluid constitutes their only appreciable difference.

The rut therefore, we say, is and does to the females of the inferior animals what menstruation is and does to human females. It enlivens and heightens in them the sexual appetite, and prepares them the better for the process of impregnation. A few details on this topic will demonstrate, if we mistake not, the truth of our position. Some of these details we derive from the effects or accompaniments of the rut in the females of our domestic animals. And they are strongly analogous to the phenomena of menstruation.

During the time of menstruation, it is well known, from ocular inspection, that the ovaries and whole generative system of woman are unusually engorged with arterial blood. So is the generative system of the cow, the mare, the ewe, and all other female animals, during the time of their rut or heat. And, during the same period, though the external organs of our domestic female animals do not discharge a genuine menstrual fluid, in the usual acceptation of the term, they discharge something strongly resembling it. The fluid is neither blood-like, nor large in quantity; but it is very perceptible to the eye, frequently witnessed, and not unfrequently tinged with blood.

Thus analogous are the phenomena of menstruation and rut. And in their effects on the female generative system, as regards its susceptibility of impregnation, they are also similar. As already stated, the females of our domestic animals, as well as of the deer, the rabbit, the hare, and others both wild and tame, are impregnated only, and admit the male only, during their heat, or rutting season. And, though woman admits the sexual embrace at all seasons, she does so with more warmth and strength of feeling just before and after the period of menstruation, than at any other. And she is then, in like manner, most susceptible of impregnation. Hence married women, whose sexual intercourse is uninter-

rupted, begin habitually their count of pregnancy from about the close of a menstrual period. And their calculation is rarely incorrect.

But wherefore does woman permit and enjoy the sexual embrace, at all times—other female animals doing so periodically, and only at long intervals? The answer is deemed obvious and easy. *Because she frequently menstruates.* On each occasion her genital system, especially the ovaries and uterus, becomes abundantly injected with arterial blood—an event which, whenever or wherever it *healthily* takes place, always refreshes the part, invigorates its vital properties, and fits it for a more energetic and efficient performance of its functions. And the intervals between the menstrual periods are so brief, that the condition of augmented feeling and vigor imparted to the genital organs, at a preceding period, is not all lost, before the occurrence of a subsequent one. Hence the constant sexual feeling of woman, and the permanency of her fitness for impregnation—marked, however, as already stated, by different degrees of these attributes, at different times.

Although we now, for the first time, publish these views on the nature, effects, and uses of menstruation, yet have we entertained and defended them, both publicly and privately, for many years. Nor, as far as our knowledge and recollection serve us, have they been published or entertained by any body else.

We claim them, therefore, as our own, assume the responsibility of any errors they may contain, and deem ourselves entitled to such credit and consideration as they may be thought calculated to confer. Their correctness receives additional confirmation from the two following facts. In woman the sexual appetite greatly declines—is at times entirely extinguished, and she ceases to be fruitful, as soon as her catamenial function disappears. Without any farther illustration or proof, therefore, we submit them to the unprejudiced judgment of the reader, not without some hope, that he will not find them altogether unworthy of his attention, nor peradventure of his approval.

C. C.

Selections from American and Foreign Journals.

Case of enormously enlarged Stomach. By JOHN FONDEY, M. D.—Professor Horner has handed us the following history of a patient, whose stomach has recently been deposited in the Museum of the University of Pennsylvania. It is of enormous size, probably without a parallel.

Lindsay S. S., aged about twenty-three, was originally from Kentucky (his native place I know not), but for some years past he resided in Tazewell County, Illinois. He was rather a wild youth, and became dissipated in his habits, drinking freely of ardent spirits, yet, although he was in the habit of indulging in very large potations, he was never known to stagger, or indicate in any manner, that he was under the influence of stimulus. In the course of a few years he became the subject of the enormous appetite which constituted so prominent a feature in the history of his disease. Soon after (the exact time I know not), he became insane, yet having lucid intervals, during which he was capable of managing his affairs, and fulfilling his civil and social relations. His insanity was of a harmless character, a prominent feature of which consisted in a propensity to appropriate to himself little articles, no matter how trifling, and he seemed to take as much delight in securing an old nail, could he only do it unobserved, as others would in appropriating to themselves something more valuable, and fitted to excite human cupidity.

The amount of food consumed by him at a meal was very large. He would eat at one meal at least two gallons of mush and milk, and other things in proportion. Sixteen large cups of coffee were about the smallest quantity of this beverage taken by him. Usually this large amount of food was thrown off by his stomach, though sometimes it was retained. He would eat several meals a day, four or five in number, all of them as large as the above in quantity.

By some physicians he was supposed to have a tape worm, by others his disease was attributed to an irritation, or subacute inflammation of the mucous membrane, or membranes of the stomach; the correctness of which diagnosis was fully shown on dissection. During his periods of insanity he was strongly inclined to wander, and was placed in the Belleville prison, in consequence of some little theft committed by him in his paroxysm. He was, therefore, kept in some degree of confinement at the house of his brother, from which he managed to escape, and came as far as Canton, near which he received the lynching which resulted in his death.

His disease had by this time produced some effect upon his frame—he was emaciated, though naturally of a good constitution. He travelled in company with an individual of his acquaintance, and when they arrived at Canton he took several keys out of the doors of different shop keepers and put them in his pocket. After they had gone a few miles from Canton, two men overtook them, and charged him with having stolen a trunk from the hotel—he denied it, but confessed that he had taken the keys, and restored them. His companion, however, who was, undoubtedly, the real thief, then affirmed that Scott *had* stolen the trunk, and said there was no use in taking him to Peoria for trial, for they knew him in that place to be a fool, and would only let him go free—that while they had him in their power they had better take the punishment in their own hands. Hereupon they took a teamster's whip, with a long thick silk lash to it, and each in turn whipped him on the back, until the lash was wholly worn out, and then cutting a thick hickory stick, they renewed their blows until they had given him about 250 lashes. He was then released. This was on Monday, December 17th, 1838, and on Sunday evening he arrived at Columbus, Adams County, the place in which I was then located. He had, I understood from some passengers who came in the same stage, eaten three or four enormous meals that day, and seemed rather wild—at times dozing, and complaining much of being half dead with hysterics. His gait was rather unsteady, and he seemed extremely weak. On entering the hotel he requested some water; after it was brought, he laid down, and in ten minutes an individual found him apparently asleep. Some time after, however, on attempting to arouse him, no answer was returned, nor could he be awakened. I was then sent for. When I saw him I found him perfectly destitute of sense or motion. Not a muscle (except

those of respiration) moved; his breathing easy, without stertor; pulse variable—now full and hard, then weak, feeble and intermittent; temperature also changeable—at one moment his face flushed, at the next pale—his legs at one moment warm, very soon cold, and then warm again. On flexing the joints, the muscles did not keep the bones in the position thus given them, as in catalepsy. All his symptoms seemed to indicate a prostration of nervous energy, and remedies were prescribed suited to such a condition of the system. He seemed at times dead, but would come to again—this continued on—all remedies were unavailing, and on Monday morning, Dec. 24th, 1838, at 2 o'clock, about eight hours after his arrival at Columbus, he died. After death, however, his eyes remained so clear and prominent, and his skin so elastic and warm, that I doubted whether he was dead, and employed means to bring him to, but on the second day he gave evidence that the principle of life had departed, and he was consigned to the grave.

Four weeks after, his brother arrived, for the purpose of having him examined, preparatory to the trial of those who had beaten him, and I was requested to conduct the dissection. The weather was cold and windy, and we were obliged to examine him in the open air; besides that, we had so little time for examination, that I was not able to make as thorough and satisfactory an investigation as I could have wished—in fact I was compelled to leave some organs unexamined, and those which were examined it was found necessary to hurry over, so that if the account of the post mortem appearances is not as full as you would desire, you will, I trust, attribute it to the circumstances in which I was placed.

The skin indicated the reception of a great many lashes, some of them superficial in their effects, others extending down to the bone, as indicated by the immobility of the skin at these points, its hardness, and apparent attachment to the bones. Extensive sugillation was also apparent on the dorsal aspect of the trunk, especially in the lumbar region and the inferior portion of the dorsal. On tearing away the calvarium, the dura mater was found to adhere to it with great tenacity—the parietal foramen rather large, and considerable blood, grumous, yet somewhat fluid, issued from the parietal vein. The vessels of the membranes of the brain, especially those of the pia mater, were rather full of blood—not more congestion, however, I should imagine, than we should look for in one who had labored for some years under insanity. The substance of the brain was very soft, whether from in-

cipient putrefaction, or from the disease, I know not, most probably from the first cause, as no red points could be discovered on making incisions in its substance. Nothing unusual was discovered in the spine, though the examination was here hurried over, from an apprehension that sufficient time would not be left for the investigation of the other organs.

On opening the abdomen, the small intestines were found to be nearly equal in size to the colon. A large portion of them was slit in order to ascertain whether any tape worm was present or not. Nothing of the kind was, however, visible—a little mush seemed to be all that occupied the intestines. No conveniences were at hand to wash the bowel and ascertain the appearance of its internal surface. The spleen and liver were enlarged; want of time prevented an examination of them. There seemed to be a total absence of fat in the parts which were examined. The thoracic cavity was not opened, by reason of the shortness of time.

As regards the appearance of the stomach, you, sir, are better qualified than myself to ascertain the changes of structure it has undergone. I will not, therefore, say any thing on this subject, but merely observe, that it was much more rubescent at the time of the dissection, than when I handed it to you.—*Med. Examiner.*

Professional Incomes in London.—An interesting Memoir of the late Dr. James Hope, the distinguished author of the Treatise on Diseases of the Heart, has been recently published by Mrs. Hope. The following particulars, in relation to the incomes of the first grade of practitioners in London, will be read with interest:—

On arriving in London, Dr. Hope was led into the belief that the first twenty physicians of the metropolis divided about £80,000 annually between them, and that a successful physician might hope to be established in good practice in about five years. To be one of so large a number as twenty seemed no difficult task, and therefore "he ignorantly hoped" that if he succeeded at all, he should be making £4000 a year in about five years! He was, of course, quickly undeceived. He had now gained many friends, he was married, he was a successul author, but the approaches to £4000 a year were "very tardy." He thought himself in fault, until a little friendly conversation with Dr. Chambers and Sir Henry Halford taught him more correctly upon the subject of so speedily

stepping into large receipts. Dr. Hope kept a regular account of every fee which he received during the twelve years he was in London. "We are, therefore," says the Editor, "enabled to speak with the greatest accuracy." The first two years he was in London he made £200 a year. The third year, the accidental removal of some families who employed him, reduced his practice to £150. At the end of the third year his work on the Diseases of the Heart was published, and he came before the profession as a physician to the Marylebone Infirmary. Still, in the first year after, his practice was little increased; but from this period his reputation became extended, and his practice gradually increased, until in eight years more, when he retired, he was making £4000 a year. This—as we should call it—great success was fairly and honorably labored for and won.—*Med. Examiner*, from *Lond. Med. Gaz.*

Bourgery on the Anatomy of the Spleen.—M. Bourgery considers the spleen to consist of ten component parts:—1. Vesicular membranes; 2. blood-vessels; 3. floating vascular corpuscles; 4. a granulo-capillary ground; 5. a splenic liquid; 6. splenic glands; 7. lymphatic vessels; 8. nerves; 9. cellular tissue; 10. the enveloping membrane of the spleen.

The splenic vesicles are uniformly spread over the whole extent of the organ, are of an irregular polyhedral, and, when fully injected, of a spheroidal or oval shape, varying in size in different animals, and also in the same spleen. In man their mean diameter is from 1 to 1¼ millimetre. Each vesicle does not form a simple cavity, its walls being traversed by vessels which form projections in the interior, covered by the lining membrane. In consequence of this arrangement the cavity is divided into cells, at the bottom of which the minute glands and capillary vessels are seen in relief. There are orifices in vesicles of two sorts:—1. Orifices of communication more or less irregularly circular, whose edges are thin, and formed by a fold of the membrane which lines the cavity. Their diameter is from a quarter to half of that of the vesicle. There are one, two, or three orifices in each vesicle; and to this reciprocal communication is due the ready inflation of the organ, not only from the veins, but also from an aperture on any part of its surface. 2. The venous orifices, less numerous than the communicating, and scattered here and there, a single vesicle sometimes containing two or three, at others one cannot

be found in a group of several vesicles. They are of a circular or elliptical shape, one-twelfth of a millimetre in diameter, and are the absorbing mouths of veins. Between the vesicles are septa, or intervesicular spaces, formed by the separation of their lining membrane, and containing the splenic glands and vessels, their size varying with the greater or less repletion of the organ, but generally bearing the relation of 2 to 3 to the vesicular part. The investing membrane, forming the walls of the cells, is continuous through the whole extent of the spleen, and may be considered as one homogeneous whole, divided into numerous little ampullæ. It is supported by vessels and glands, and forms a sort of frame-work to the organ. This membrane is very complicated in its structure, and cannot therefore be considered, as by Malpighi, to be a simple dilatation of the internal tunic of the veins.

The splenic arteries and veins, running side by side, are directed towards the periphery of the spleen, the veins being pierced in their whole extent by little circular apertures, leading into the smaller intervesicular branches, which are distributed to the splenic glands and the membranes of the vesicles. The small vessels project into the cavities of the vesicles in a peculiar manner to reach the floating corpuscules, and present the appearance of a bunch of grapes. Lastly, all the small vessels of the spleen, in the turgid state, present numerous dilatations and contractions, so as to give them a marked knotted appearance.

The corpuscules are small bodies floating in the cavities of the vesicles, to the walls of which they are attached in a pediculated manner, by the extreme branches of the blood-vessels and lymphatics. They are about fourteen or fifteen times the size of the blood-globules. By the granulo-capillary basis is intended to be represented the two structures placed beneath the vesicular membrane, viz. spherical pale granules, or glands, four or five times the diameter of the blood-globules, and the capillary net-work of veins, arteries, and lymphatics.

The splenic liquid, which appears to be elaborated by the floating corpuscules and the granulo-capillary basis, is deposited in the cavities of the vesicles, or taken up by the absorbing veins or their walls. It is thick, viscous, of a reddish-brown color, and under the microscope appears to consist of globules suspended in a yellowish unctuous fluid. 1. Lenticular globules, some of which are surrounded with a red border, and appear to differ very little from ordinary blood-globules; others being colorless. 2. Whitish globules, irregular in form and size, and resembling those found in the chyle and lymph.

The splenic glands, so far as actual volume and consistence are concerned, form the principal organic element of the spleen. They fill, with the ramifications of the vessels, the intervesicular spaces. Their diameter is a quarter of a milli metre. They are united by bands of a similar nature with themselves, which extend over every part of the spleen. From the afferent and efferent lymphatic trunks, and the dispositions of the vessels about them, M. Bourgery is convinced that they are simply minute lymphatic glands. The lymphatic vessels are extremely numerous in the spleen, and in many situations remarkable in their structure, being enlarged at intervals, and, in addition to their valves, divided in their interior by septa, which gives them a sort of rudimentary glandular structure, and shows them to be not merely canals for transport, but also, in some degree, organs of elaboration.

M. Bourgery considers the spleen as an apparatus for the elaboration of the blood, and draws an analogy between it and the lymphatic glands; observing that if, in relation to its anatomical structure, we may define the spleen as a vast lymphatico-sanguineous gland, on the other hand, lymphatic glands generally may be considered, to a certain extent, as small spleens appended to different parts of the circulatory apparatus. In treating of the intimate structure of these glands, we shall see how far the idea of the conformity of these two sorts of organs, evident so far as the glandular structure of the spleen is concerned, is borne out by the internal organization of the canals of the lymphatic glands.—*Ibid, from Gazette Médicale.*

Ligature of the External Iliac Artery for Femoral Aneurism. By Wm. Power, M. D., one of the Attending Physicians to the Baltimore Alms-house Hospital.—Christopher Daly, a native of Baltimore County, æt. 61, entered the surgical ward on the 1st of July; a man of stout frame, very youthful appearance for one of his years, and enjoying excellent health with the exception of the aneurismal disease for which he came to the hospital. For the last fourteen years he has enjoyed good health, uninterrupted by an attack of any sort unfitting him for his daily labor. About fifteen months ago, according to his best recollection, his attention was first accidentally called to the aneurismal tumor whilst digging in a ditch, and standing in water, though with his feet well protected. Placing his fingers upon it, he says he "distinctly felt

a pulse, but thought it was in a very strange place."—As it gave him no pain or inconvenience he thought it was nothing serious and continued to work on as usual. About a month had elapsed from this time, when he was suddenly seized with violent cramps in the diseased limb, and finding exertion painful, he stopped his usual employment, but assisted during the following harvest in the lighter kind of field labor. The limb was slightly swollen below the tumor when he first discovered it; but after the attacks of cramp, which were from time to time repeated, the swelling increased, together with a sense of numbness diffusing itself over the whole leg. The tumor has steadily continued to increase, together with the œdema of the leg. He has, for the last eleven months, kept quiet and strictly avoided all exertion, and has undergone no treatment until he came under our care. The aneurismal tumor was situated in the right groin immediately below Poupart's ligament, which was forced a little upwards in an arched form. It was of an irregularly rounded form, about three and a half inches in its perpendicular, and three in its transverse diameter—pointing a little upwards and inwards, where the pulsation was more distinctly seen and felt than elsewhere. Its pulsation was evident to the eye—communicated a distinct thrill to the hand when laid upon it, a *bruit de rape* to the stethoscope, and the pulsations were eccentric. Compression upon the iliac artery, on the psoæ muscles, arrested all pulsation in the tumor. The external iliac for an inch above the tumor felt larger than the artery of the opposite side, and both here and in the continuation of the femoral below the tumor, gave a distinct file-sound to the stethoscope. The lymphatic glands of the groin were swollen and hard—the leg was one-third larger than that of the opposite side—purplish in hue, one degree higher in temperature, affected frequently with cramps, and pain with a sense of numbness over the knee and instep.

Tongue clean, appetite good, bowels regular, skin pleasant, pulse 80—natural, sounds of the heart clear and distinct, chest every where resonant, respiratory murmur vesicular, no ossification of the arteries at the wrist. The rest and quiet of the hospital reduced in a degree the œdema of the leg, and caused the tumor to appear more prominent. He was placed on mild diet, and one or two gentle aperients administered. His health remained undisturbed, and his spirits good up to the 19th of July, when, assisted by my colleague, Dr. Annan, and in the presence of several medical friends, the following operation was performed.

An enema having been previously administered, and gutt. lx. Tr. opii, given an hour previously, the patient was placed upon upon a suitable table, his pelvis inclining towards the left side, and an incision made through the skin and common integuments of three inches and a half in length, extending from three-fourths of an inch to the inside, and on a level with the antero-superior spinous process of the ilium, with a curve presenting downwards to about three-fourths of an inch above the external ring—a small vessel running to the skin was divided in the inner angle of this wound and secured. The glistening fibres of the fascia of the external oblique were then divided, according to a suggestion in the last edition of Dr. N. R. Smith's work on the arteries, in the same direction, but a half inch above Poupart's ligament. I now attempted to pass my finger under the edge of the external oblique and fascia transversalis, but found the border of the fascia of the external oblique presented such obstruction to free manipulation, by its resistance, that, to facilitate my farther progress, I was obliged to divide it with a buttoned bistoury, thus making a T incision in this fascia. The finger now passed readily down on the outer side of the internal ring, avoiding the cord, and after a little dissection with the nail, came in contact with the artery. Fearing, however, the soundness of the coats of the vessel thus low down, the sac of the peritoneum was gently pushed upwards and separated from the fascia transversalis for an inch and a half; and this membrane, together with the fibres of the internal oblique and transversalis divided by a buttoned bistoury on the finger for three-fourths of an inch. This sufficiently enlarged the wound for easy manipulation. The finger sought the artery about three inches from Poupart's ligament, and with one of Prof. Gibson's needles, armed with a ligature of two strands of sadler's silk, waxed but not twisted, an attempt was made to pass it from without inwards; finding the curve of the needle so great that I could not depress the handle sufficiently on the abdomen, I cautiously separated with the nail the vein from the artery, and passed it from within outwards. The ligature was then drawn tight with some difficulty at this depth, my assistant pressing on the knot until the second was tied. The pulsation immediately ceased in the tumor. The wound was brought together by two points of suture and a few adhesive straps. The operation lasted 25 minutes, and was borne extremely well by the patient. The patient, after the wound was dressed, was carried into a well-ventilated room, and placed on a bed with the limb of the side operated upon well flexed on the pelvis.

Complains only of fatigue and numbness of the right limb, with dull pain of the loins and hypogastric region—pulse 80—natural—at 7, P. M. Temperature of right foot, as tested by the thermometer, seven degrees lower than that of the opposite side—gave Tr. opii, gutt. xxx.

July 20th. Patient rested tolerably during the night—slept nearly four hours; countenance cheerful; some nausea and anorexia. Pulse 80, as yesterday; skin pleasant; complains of dull pain in the loins, and some cramps in the leg. Tumor decreased one-third in size; hard on its external side; slight pulsation at its inner and upper portion. Right leg florid, only two degrees lower in temperature than the left; ordered venesec. 14 oz. ℞. Morphiæ acet. gr. ij.; aquæ camphor. ℥iv.—dessert-spoonful every two hours. Towards evening his pulse rose to 114. Takes barley water made cold with ice.

21st. Appearance much altered; countenance anxious, haggard; eyes sunken and suffused; voice weak; nausea, constant retching; complete anorexia. Tongue reddish along the edges; thirst; skin bathed in perspiration; pulse 140, small; abdomen tympanitic; complains of sharp pain along the recti muscles, in the iliac region and about the loins; bladder irritable, trying to make water every ten minutes; venesec. oz. 14. ℞. Calomel grs. xv., pulv. opii, grs. ij. Added two drops of hydrocyanic acid to each dose of the solution of morphine; forty good leeches applied to the abdomen. At 7 P. M. the leech-bites were still running freely, and had afforded great relief; pulse 130; blood drawn this morning well buffed; still restless. Continue anodyne. Tumor as yesterday. Temperature equal on both sides.

22d. Countenance much improved; pain and uneasiness much less; abdomen still tympanitic; no movement of the bowels. Pulse 125, fuller but soft; tongue moist; thirst less, and asks for food. Still complains of pain in the back and abdomen; bladder still irritable; venesec. 8. oz. ℞. Cal. and opium as yesterday: hop fomentation over abdomen. 7. P. M. Still improving; skin pleasant; demands food; no passage; blood drawn in the morning lightly buffed; the bleeding made a sensible impression upon his pulse. ℞. Cal. grs. viij.; opii, gr. j.; chicken water, and milk iced. Continue solution.

23d. Passed a good night, and makes no complaint this morning excepting of his bladder; abdomen much softer; skin pleasant. Pulse 96, soft; tongue clean; no stool; little thirst; wound gives no pain; an erythematous blush surrounding it for some inches; complains of the limb. ℞. Sal Rochelle,

℥j.; aqua, ℥viij.: wine-glassful every hour. 7 P. M. Bowels not moved; complains of flatus. Ordered a simple enema.

24th. Looks as well as before the operation; makes no complaint; pulse 90, soft; tongue clean. Peritonitis fairly subdued, and we looked forward to a happy result; waking from a sleep at one o'clock he turned over in bed; his nurse a few minutes after was struck by his respiration and altered facies, and a quantity of blood in the bed. I saw him a few moments after, pale, restless, pulse scarcely perceptible, almost unconscious, his bed floating in blood—between two and three quarts. The hemorrhage had ceased; a compress was laid over it; nor did it return; he sunk rapidly, and died about 7 P. M. It was not deemed advisable to make any effort to secure the vessel, as the slightest shock would have determined dissolution under our hands.

Autopsy eighteen hours after death. Weather very warm. Head and face purplish; features swollen; bloody serum exuding from eyes; slight greenness about the clavicles and abdomen; rigidity considerable; muscles large and firm. The lips of the wound were united, excepting for a half inch on either side of the ligature; this space occupied by a clot of blood; the points of suture had held firmly, and there was no disturbance of parts.

The abdomen was opened by incisions from the umbilicus to the pubis, and to the edge of the ribs. Their flaps turned down exposed to the peritoneum. There was no effusion of serum; no adhesions; but over the tract of the artery, extending upwards towards the kidneys, and down into the pelvis to where the peritoneum invests the bladder, this membrane, in a space equal to the size of the hand, was of a bluish livid tint, more wrinkled than elsewhere, as if lightly parboiled, presenting numerous whitish yellow points, which could be scraped off with the nail; some flakes as large as a half-dime. The membrane was firm; no adhesion with the intestine. The peritonitis had been local, and was entirely subdued. The adhesions of the lips of the wound were then torn open; the skin and cellular tissue were the only parts adherent. The blood confined in the wound had insinuated itself upwards for some distance between the fascia transversalis and the peritoneum, as well as between this membrane and the fascia iliaca; about four ounces of clotted blood were removed from the wound. The artery was then exposed, and found with the ligature around it; when pressed upon, blood came from an opening immediately under the knot. It was then carefully dissected out and removed, from Poupart's ligament to

the bifurcation of the aorta, and laid open, as well as the external iliac of the opposite side. They were equal in size; at least one-third of an inch in diameter; the inner coat of a creamy yellow hue; a little rough. The inner coat, where the ligature had been applied, on the posterior half of the artery, was wrinkled and crimped, so as to be smoothed out with some difficulty, but was not cut through, nor was there any effusion of lymph. On the anterior part was the opening, at least one-half the circumference of the vessel. The edges a little ragged, of a purplish hue, within a quarter of an inch of the opening; friable, and breaking easily under the nail. The outer coat was livid to the same extent; softened and more friable than the rest of the vessel; a soft clot below, but none above the ligature. The vein perfectly sound; no further separated from the artery than by the needle in passing the ligature; this was applied about an inch and a half below the bifurcation of the common iliac, and about the same distance from the origin of the epigastric. The tumor in the groin was next laid open and cleansed of its clots. It was about three inches in each diameter, the anterior wall formed by the fascia lata, its lateral and posterior walls by the muscles of the thigh; irregularly anfractuous. The profunda, which was given off very high up, immediately under Poupart's ligament, formed a portion of its posterior boundary. The artery, for an inch above where the tumor commenced, was filled with osseous concretions, grating under the scalpel, and pricking the fingers. The continuation of the superficial femoral where it emerged from the tumor, from the number and regularity of the circular atheromatous bands, looked more like the trachea than an artery. The aneurismal tumor was very thin and prominent towards the upper and inner part of the thigh. It contained between four and six ounces of soft black clotted blood, and as much firm fibrin arranged in layers penetrating into the anfractuosities of the tumor.

The arterial system was everywhere well developed; these vessels having more than their usual calibre. The heart was large and flabby, its walls thin, a little fatty; the valves all sound; aorta not dilated; other viscera of the chest and abdomen healthy. Head not examined.—*Maryland Med. and Surg. Journal.* September, 1842.

Counter-irritants.—The following formulæ have been communicated by Dr. Turnbull to the editors of the Pharmaceutical Transactions:—

Tinctura Capsici Concentrati.

℞. Capsici Baccarum, ℥iv.
Spiritus Vini Rect., ℥xij.

Macera per dies septem et cola. (It may also be made with advantage by displacement.)

This concentrated tincture is used as an external application, and is found to be a powerful rubefacient and counter-irritant, for which purpose the ordinary tincture of capsicum is not sufficiently potent.

Veratria, dissolved in this tincture, acquires increased activity; the capsicum apparently facilitating its absorption into the skin. Four grains of veratria, dissolved in an ounce of the concentrated tincture of capsicum, will be found as powerful in its effect as twelve or fifteen grains dissolved in alcohol.

Pulvis Aluminis et Capsici.

℞. Aluminis Sulphatis, partes tres.
Tinct. Capsici concentrati, partem unam.
Misce et sicca.

A very small quantity of this powder, applied to the tonsils, is found more efficacious, in some cases, than an alum and capsicum gargle.

Unguentum Ipecacuanhæ.

℞. Pulveris Ipecacuanhæ, ℥ij.
Olei Olivæ, ℥ij.
Adipis, ℥ss.
M. ft. unguentum.

Unguentum Emetinæ.

℞. Emetinæ, g. xv.
Sp. Vini. Rect. q. s.
Adipis, ℥ss.
M. ft. unguentum.

Dr. Turnbull states, that he has found this ointment particularly efficacious as a rubefacient in pulmonary and rheumatic affections, producing little or no pain or inconvenience to the patient.—*Med. Exam.*, from the *Medico-Chirurgical Review*, July, 1842.

Abdominal Tumors.—It may seem strange that the diagnosis of abdominal tumors, which manifest themselves to the touch, and to the sight, should be so difficult and puzzling as it often is. I mentioned some reason for this before: the loose and shifting manner in which some of the viscera of the belly are packed and fastened; their liability to enlarge beyond their natural limits; their accidental dislocations under disease. It would be in vain to attempt even a sketch of the infinite variety of these deviations from the healthy state. Every case of abdominal tumor forms a separate object of study, and must be judged of by its proper circumstances. All that I can profess to do, is to offer you some rough hints on this interesting subject.

Some kinds of tumor result from morbid growths; such are all the varieties of cancer: some from the presence and multiplication of parasites; of which we have examples in collections of hydatids: some are produced by the distension of hollow organs; as where concretions, or fæcal matters, oras seg, lodge in the intestine: some consist in the mere enlargement of parts.

Let us enumerate the principal of these, that you may know what chiefly to expect.

1. There are, I say, tumors from lodgements in the *bowels;* and these are more hopeful than most kinds of abdominal tumors. Sometimes the stomach, or part of the intestinal canal, is distended in consequence of a mechanical impediment to the course of its contents: and this impediment may be invincible.

2. *Ovarian* tumors are very common. Of these I spoke at some length in a former lecture.

3. The *liver* is very liable to enlargement: either from simple congestion of blood; or from the interstitial deposit of adipous matter; or from the intrusion of malignant growths; or from colonies of hydatids.

4. So also the *spleen* swells, from fulness of blood, or from specific deposits in its substance.

5. The *kidneys* sometimes attain a vast size; being occupied by malignant disease, or swollen by pus that finds no vent.

6. Enlargements of the *mesenteric glands;* cancerous degeneration of the peritoneum, especially where it forms the *omentum;* tumors connected with the *uterus;* aneurisms of the *aorta;* constitute other species of abdominal swelling, which I simply mention without further comment.

Now our judgment of the character of a given tumor is naturally influenced by its *place*. In the right hypochondrium, we suspect the liver; in the left, the spleen; in the epigastric region, the stomach; in the hypogastric, the womb; in either flank, an ovary, or perhaps a kidney; in the track of the colon, we guess at fæcal collections.

But sometimes the situation of the tumor fits more than one, or than two, suppositions. Between the ribs and the ilium on the right side we may have an enlarged ovary, a tumid kidney, a distended cæcum. A prominence in the epigastrium may be due to cancer of the stomach, to an infarcted transverse colon, to a ventral aneurism. Above the pubes, the distended bladder, or the enlarged uterus, may equally project. The sigmoid flexure of the colon loaded with fæces, the left kidney exaggerated by disease, a bulky ovary, may either of them occupy the same sinistral space.

Moreover, the colon deviates strangely, and not seldom, from its natural course and position; and the magnified viscera may invade, by their displacement, or by their irregular expansion, the regions that are proper to other organs.

Our conjectures are assisted by the associated symptoms, and by observation of the regular performance, or of the disturbance, of particular functions. Yet here, also, we meet with continual sources of fallacy. Pressure from a tumor without may, as well as infarction within, impede the passage of alimentary matters through the bowels, of urine through the ureters; and cause, in the one case, flatulence and tormina—in the other, retention or suppression of urine. Growths foreign to the liver may, nevertheless, press upon its excretory ducts, and occasion jaundice. And so of other parts and functions. I mean, that the functions prominently deranged are not always the functions of the part occupied by the tumor, but of organs which are secondarily and accidentally subjected to its disturbing influence. Your sagacity will be abundantly tried in balancing the evidence of different symptoms in these obscure, yet palpable, forms of disease: and after all, you will often doubt; and often, when you do not doubt, you will mistake.

Enlargement of the liver may usually be distinguished from other tumors of the right hypocondrium, by *percussion*. Try from the clavicle downwards. At first, you get a hollow sound. Then, a little below the nipple perhaps (for the spot varies much in different subjects), the sound begins to grow dull. If this dulness be traceable, without change or interruption, to the tumor, the inference is strong that the tumor is hepatic.

Any other tumor there situated leaves, most commonly, when the patient is recumbent, a palpable sulcus above it; or a space in which the sound, upon percussion, is different from that which is yielded by the liver.

Percussion helps us to discriminate an ovarian from a renal tumor. When the swelling is large, the intestines lie behind the one, in front of the other; and the sound is affected accordingly.

Tumors that are readily *moveable*, are generally intestinal, omental or ovarian.

A pulsating tumor is not necessarily an aneurism. The healthy artery will lift almost any sort of hard swelling that happens to lie directly over it.

The occurrence of hæmatemesis or melæna would corroborate your belief that a tumor in the right hypochondrium was hepatic—in the left, was splenic.

Even when you are satisfied as to the organ affected, there comes another question, scarcely, in some cases, less difficult than the first—What is the *nature* of the tumor?

Suppose, for the sake of illustration, that our inquiry relates to the liver. If the tumor be large, smooth, roundish, of slow growth, and the general health is not materially deranged, it is, most likely, an *hydatid* tumor. If along the edge and upon the surface of the augmented liver, you can feel large inequalities and projections, and if the complexion and general state of the patient are expressive of failing health, the enlargement is, in all probability, *cancerous:* and if there be other traces of carcinoma in the system, this conclusion becomes almost certain. Small, hard irregularities betoken the *hobnail* liver, which is sooner or later accompanied by ascites. When, without pain or jaundice, the liver of a phthisical patient transgresses its natural boundary, it is, in all probability, *fatty* liver.

By applying a similar method of investigation to other ventral enlargements, you may frequently hit the right scent, and trace the mischief to its true source. To treat the subject in detail would require a volume. I may refer you to a series of papers by Dr. Bright, in the Guy Hospital Reports; where you will find a host of examples, and much valuable information, concerning the most common and the most important kinds of "abdominal tumors and intumescence.—" *Watson's Lectures.*

Use of Moxa in Rheumatism. By JOHN LENEY.—The laboring classes in Ireland, from exposure and insufficient clothing, are very aubject to local rheumatism. Rheumatic fever is a disease comparatively rare among them. Those forms of most frequent and distressing occurrence are lumbago, rheumatism of the deltoid muscles, and sciatica. In obstinate cases of local rheumatism I invariably apply moxa; there is no application so immediately successful, or which creates less inconvenience to the patient, especially if he be a laborer. At dispensary my ordinary treatment for lumbago is castor oil and turpentine draught, as follows: ℞. Olei ricini. ℥j.; terebinth. syrupi, aa ℥iij.; aq. menth. piperit. ℥j M. Ft. haustus.

The turpentine epithem on flannel to the loins, kept on from twenty minutes to one hour, or until, in fact, the patient exclaims loudly for its removal; it often brings out very minute vesicles, and the cuticle desquamates. Also ten or fifteen grains of Dover's powder at bed-time, the feet and legs bathed in warm water.

In rhematism of the deltoid muscles, and sciatica, apply moxa by all means, as blisters and tartar-emetic ointment disable the patient, and render him incapable of working. The laboring classes cannot afford to be confined to the house, whereas after the application of moxa the patient may return to his work at once. I shall instance a few aggravated cases in which it proved successful.

James Pender, ætat. 56 years, got sciatica from standing in water, the principal part of a day, washing sheep. He had been confined to the house, nay, to the bed, for six weeks, and had used a variety of means—stupes, blisters, warm plasters, &c., without deriving the smallest benefit. I applied moxa in three places; first, at the origin of the nerve, each application three inches distant, and following the course ot the nerve down the limb. This man had eighteen applications. From the first burning the disease descended, so that I had to continue it down the limb to the ankle. Several of the burns continued open for some time. One particularly down the calf of the leg discharged pus for three weeks. This man has been ever since (now five years) at labor without having had any return of the disease.

Judy Cavanah, æt. 57 years, confined to bed six weeks from sciatica of right limb: was burned in four places, on those parts in the course of the nerve which she indicated as most painful. She was immediately cured. That was three years

ago. Last winter she had the disease in the left limb, was confined to bed, and suffered excruciating pain. The moxa again relieved her, being applied in seven places. She had not any internal medicine during either attack.

Bridget Doyle, æt. 48 years, living at a distance of three miles, being informed of her sister's recovery, was brought to the dispensary on a car, and carried in, being unable to put the limb under her; had been confined to bed for fifteen weeks. This woman was burned in three places; and so immediate was her recovery, that she walked home with her husband in two days after. He, laboring under rheumatism of the deltoid muscle of the right arm, which rendered him incapable of working (having been employed in threshing corn), was burned in two or three places, which enabled him to return to his work the following day.

A stout, able-bodied young man, who had undergone a variety of treatment, was blooded, blistered, purged, used sudorifics, and finally had tartar-emetic ointment rubbed in throughout the whole limb, from which he had no relief; from this unlimited application of tartar-emetic ointment, a mass of tubercular swellings remained in the skin, so thick that it was difficult to procure places for the application of moxa; he was burned in three places, and became immediately well, now two years since. He has had no return. I selected the foregoing cases to show its permanent benefit.

June 15th, 1842.—Jane Gaskin, æt. 14 years, has had sciatica of the left limb for five weeks, and been confined to the house from pain and inability to move the limb (the limb is smaller than the opposite). Moxa applied in three places, from the ischium to popliteal space; lost all pain immediately, and was able to return at once to her ordinary avocations. This girl had been blistered, used tartar-emetic ointment, aperients, &c., without any benefit; has had no return of pain up to this day (June 30th).

My experience would lead me to say that if there be not an immediate relief obtained on the first or second application, the case will be tedious, or pass down the limb, as in Pender's case. I am at present treating a case exactly like Pender's, in which the pain has left the parts first burned, and is now taking the course of the posterior tibial nerve.

The way in which I make moxa, and apply it, is the following:—In a strong solution of nitrate of potass, soak a piece of lint, then dry it, cut off pieces the size of the thumb-nail, fasten them at one extremity over the seat of pain by a little adhesive plaster, set fire to the opposite extremity, then blow

on it strongly with a blow-pipe. The pain during the operation is very severe, causing an immediate puckering of the cuticle. Those cases did best, in which, under its influence, a general perspiration broke out.

Had I not mislaid my note-book, I should have been able to have detailed these cases more accurately, and given some cases of affections of the knee and spine in which it proved successful. For the last six years I have been using moxa with undeviating success, and cannot instance a case in which it failed. When first proposed, the idea of being burned was such that it prevented many submitting to it; but now that they have had demonstrative proof of its efficacy, they gladly submit, preferring it much to the application of blisters.—*London Med. Gazette.*

M. Velpeau on Cysts of the Eyelid.—I would say a few words respecting the case of a man, on whom I am about to operate for three small tumors on the eyelid. Of two of these the outline cannot be distinguished, being masked by the third which is more voluminous. This last is evidently a cyst filled with a fluid or semifluid material. The nature of the contents of the two others it is not easy to diagnose, from their size and the thickness of their walls, but from analogy we may suppose them to be filled with a liquid or semiconcrete matter.

Tumors of this description are very rarely cured by any plan of treatment except operation. Occasionally, indeed, they disappear without any operation or treatment. Sometimes this may result from the particular constitution of the individual, as in a young subject who about the age of puberty was freed from an affection of this nature. Or, it may be produced by some other disease concurring at the same time, as in the case of a woman who had three of these tumors on the eyelid, and on whom I had fixed a period for operation. But she came to me some months afterwards, and informed me that she had suffered under an affection of the chest, and the tumors had disappeared. Occasionally they disappear under local applications for their resolution, but we can rarely expect success to attend this treatment, and it is altogether ineffectual when they have attained a considerable developement. They may, however, be safely and with certainty cured by surgical means.

Of the two proceedings, extirpation and incision, the former, employed by most surgeons, appears to me a tedious operation. It is necessary to dissect with caution, and if it be a

cyst, whatever care is taken, it will be difficult to avoid opening it; it then is necessary to excise it entirely, and to apply cauterization, or there will be a considerable chance of return. If the contents of the tumor be concrete, the operation is comparatively easy; but, otherwise, I think incision is preferable: it is less painful, and quite as successful. Some persons in practising this operation hold the tumor with the fingers, others place an elevator beneath the eyelid. I employ simply two pair of forceps, of which one is held by an assistant; I myself, holding the other in such a manner that the eyelid is rendered tense between them, and raised from the globe of the eye. I make the incision, expel the contents, and apply nitrate of silver to the interior of the cavity. There is sometimes slight consecutive inflammation, and the eyelid swells a little, and soon an eschar is detached and the cure accomplished in eight days. A piece of lint dipped in saturnine lotion or cold water is the only dressing employed. I should, however, observe, that by extirpation the cure is more prompt; a piece of dressing is placed on the wound, and the whole is terminated.

After these observations the operation was performed: M. Velpeau incised the different tumors and cauterized the bottom of each wound.—*Lond. Med. Gaz., from a Clinical Lecture by M. Velpeau, in Gaz. des Hopitaux.*

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FLUOR SPAR.

Dr. E. R. Roe, of Shawneetown, Illinois, wishes to inform scientific gentlemen through our journal that he can supply orders for *Fluate of Lime*, addressed to Drs. Roe & Badger at the place of his residence, if properly accompanied. He writes us that, since leaving Louisville last spring, he has received requests from various gentlemen to send them specimens of this mineral. He is prepared to furnish specimens to as many as may desire them. The mines are within about 20 miles of Shawneetown, in a wild, unsettled region of country. He says he has "spent some time and much pains in collecting specimens beautifully crystallized, and tinged with every shade of lilac and pink, up to pure transparency." Dr. Roe is a gentleman of science and industry, and may be depended upon for the faithful execution of all that he proposes. Y.

"REPORT ON THE MEDICAL COLLEGE" AGAIN.

In our last number we noticed briefly a "Report" made to the City Council on the subject of the Medical College, in which the conduct

and motives of the Faculty were assailed, and which, after having been rejected by the Council, was published and sent abroad over the country. We stated at the same time that the Faculty were preparing a reply to the unfounded statements and unjust imputations contained in this document. That reply is before us now, in a pamphlet setting forth the "origin and present condition of the Louisville Medical Institute." It is a plain, unvarnished statement of facts, temperate in its tone—very temperate indeed under all the circumstances. As copies of it have been sent to the patrons of the Journal, we think it unnecessary to indulge in copious extracts from it, as was our intention, and shall therefore only advert briefly to one or two points connected with the matter.

The Dean, in the pamphlet before us, dives at once to the source of this movement, and of course looks beyond the Committee. He says:

"It is known to the citizens of Louisville that there is a party in the city who have for some years been endeavoring to produce discontent with the management of the Medical Institute of Louisville. The Managers and Professors have borne their repeated expressions of dissatisfaction, publicly and privately made, and their attempts to take the school out of their hands, with quite as much patience as is commonly shown in similar circumstances. Some indeed have manifested a natural and justifiable indignation at these unworthy efforts and schemes; but they have avoided a public expression of it, and no public defence has yet been made. They have felt extreme reluctance to a controversy with men who appeared to be proceeding from outrage to outrage, in hope of provoking one, from which they hoped to gain notoriety at least, and in which they had little to lose. But inasmuch as these men have at length succeeded in misleading some of the Council so far as to have their views presented to that body in the shape of a report, which has thus acquired some appearance of importance; although the report was rejected, it has been thought advisable by the Faculty, to place the matter in its true light before the friends of the school, some of whom may be misled into believing the statements made in the report.

* * * * *

Indeed the temper of the rejected report and the character of its statements are such as to lead the Faculty to the conviction that the enmity which prompts these movements is not to be mitigated by time nor softened by forbearance; and some extraordinary acts of those who can no longer be considered in any other light than that of determined enemies of the school as now constituted, as well as the inquiry of a number of citizens, have led them at length to the opinion that it is time to check the influence which has misled some of the Council, and which left unnoticed might mislead others.

* * * * *

These men indeed sometimes deny that they have any intention, in these efforts to obtain possession of the management of the Medical School, to disturb the present professors. But it is not to be believed that, if after years of intrigue they could by any possibility get the administration of the school into their hands, they would cease until they attain their main object, openly avowed two years ago, the possession of the chairs. It is not to be believed that men who have arranged a plan for delivering lectures, and have asked for the use of the Medical Institute for the purpose, would fail to take possession of it if they should dispossess the Managers whose sense of duty and propriety compelled them to deny them. It is evident that the object was, by succeeding in carrying the report, which they without foundation supposed would give them possession of the Institute, to do that which they openly proposed to do in 1840, eject the present Professors.

Such are the motives which led to the statements in the rejected report, and to the improper use that has since been made of it. * * * And so completely have the members of the committee entered into the feelings of the inimical party, that, according to the statement of the editor of the Gazette, which has not been contradicted, they had a great number of copies of the report printed, with the ayes and noes attached, in a form evidently intended for distribution; and one of the committee has stated to a gentleman, although he was told that the information was sought for in order to be used, that the publication was made in consultation* with Dr. Bell, Dr. Bullitt, and Dr. Flint, and from the interest they manifested in it he supposed they would be willing to pay a part of the expense if called on."

As it respects the transfer of the Square, buildings, &c., from the occupancy of the Medical Institute, which was the ostensible object of the committee, the report, so much of it as is devoted to that object, proceeds on the assumption that the Louisville College is the corporate body alluded to in the deed of trust by which the Managers of the Institute at present hold the property. Now the rejoinder of the Faculty makes it about as plain as words can make it, that this College and its trustees were no more in the minds of the City Council or of the people at their public meeting than an Indian wigwam and its inmates!—not a bit more! No reference was made to them in any way, nor can the language of the indenture or the resolutions of the citizens, by any legitimate philological torture, be made to refer to them. And yet this "elaborate report", whose statements, according to the editor of the Louisville Gazette, are "undoubtedly authentic", says, "nobody

* The member of the committee here referred to, has since stated in one of the city prints, that the expression "made in consultation with," &c., is a misapprehension.—Ed. Jour.

thinks nor has thought of establishing any other school in Louisville!" The following extract from the exposition of the Faculty contains a summary of the evidence offered by them on this point, and shows on what grounds the transfer is to be made.

"The proposition to request a transfer of the management of the school from the present Board, rests on the ground of an asserted right arising from the donation of the Council.

The right of management was in the Board of Managers of the Medical Institute of Louisville years before the donation was made. It was the knowledge of the powers they possessed which led the Council to make the donation, and was the avowed ground on which it was made. The donation therefore gives no right of management to the Council.

What then are the legitimate claims of the Council upon the school? They are those only which arise out of the terms of the donation when accepted: for it is the acceptance of a donation which subjects the receiver to the restrictions contained in the terms of the gift. Beyond these the claim of the donor does not extend.

In the present case the terms of the donation which was accepted by the Managers were these, beyond which the Council have no claim. 1. That the property should be used by the Managers for the purpose of carrying into effect their previously possessed powers of establishing a medical school: and, 2dly, that in the event of the Council's carrying the views of the citizens fully into effect, by erecting other college buildings on University Square, and obtaining a charter for a university, the Managers are to transfer, if the Council request it, the square, buildings, &c., to the trustees of the university.

The first authorises them in case the property is used for any other purpose than that of a medical school, to reclaim possession of it. But as long as it is used for the purpose for which it was given, they cannot move in the matter. How far a great failure notwithstanding the efforts of the Managers to raise up a school sufficient to satisfy the expectations of the donors, might authorise the latter to interfere, need not be considered; seeing that in the present case the utmostexpectations of the whole city have been far exceeded.

The second authorises the Council to build other college buildings on University Square, and having obtained a charter, to require the Managers of the Medical Institute to transfer the square, &c., to the trustees named in it. Before this is done there is no ground for the request.

In case however that the wishes of the citizens expressed in their meeting in 1837 should be carried into effect and buildings be erected for the other departments, and the managers of the Medical Institute should be called on to reconvey the square, &c., it is the property only that is to be reconveyed, by right of the reservation in the deed of donation; which can go no farther than to call back what was

granted; viz. the property, not the right of management, which was in the Board of Managers before the donation was made.

When that time shall arrive it will be a matter of consideration on what terms the Medical Institute of Louisville, called by anticipation the medical department of the intended university, shall really become so. These will no doubt be such as shall be agreed to by those under whose charge it has increased beyond all expectation and beyond all precedent, and in all probability will continue to increase."

One of the most calumnious charges made against the Faculty by the authors of the rejected report, is that of having embezzled the funds of the institution. But a small portion, they say, of the monies derived from the matriculation fees have been devoted to the purposes of the school, "the residue having gone with all the other revenue of the establishment into the Professors' pockets." So far from this being the case, it appears from a detailed statement of the expenditures given in the pamphlet, that "the expenses of the School have exceeded by several thousand dollars the amount of matriculation fees received;" in fact, that the Faculty have taken money out of their pockets to the amount of $8,000 over and above that fund.

But perhaps the most perfectly gratuitous suggestion contained in the report, and at the same time the one most ruinous and destructive to the school, is that the proceedings of the Board of Managers have been to such a degree informal "as to vitiate and nullify in a legal sense all their recent acts"; and of consequence, that "the appointment of professors, the financial arrangements of the concern, the distribution of collegiate advantages, and the conferring of degrees, are done without authority of law, and of course are invalid and worthless to all interested." "It appears," says the report, "that there are still living three of the original corporators of the Medical Institute, who have neither participated, nor been summoned to participate in any of its transactions for the last four or five years—a portion of the associates having proceeded to exercise all the corporate functions, without any regard to the equal rights of the excluded members."

To this most injurious statement, which strikes directly at the very existence of the institution, and yet is made under the garb of the most disinterested friendship, the Dean, after directing attention to the early history of the Institute as previously given by him in the pamphlet, and which shows the utter groundlessness of the assertion, replies more particularly as follows:

"The three surviving *members* here spoken of, are medical men, who nine years ago excluded themselves from all participation in the management of the Institute, by passing the regulation that no medical man should thereafter act as manager. This, being managers at the time, they had full authority to do by the act of incorporation in 1833; and the Legislature confirmed it by the act of 1835 as the permanent law of the school.

'The portion of the associates (spoken of) who proceeded to exercise all the corporate functions,' are four still surviving managers of that Board, which the old three abovementioned (and their then associates) constituted nine years ago; together with those who have been since elected. In electing new Managers instead of those whose places were vacated by death, resignation or removal, they were debarred from choosing any of the surviving three medical men, by the law of the Institute made by these three themselves: and were compelled to appoint the same men managers because there were no others eligible, as the three had themselves arranged the matter when they brought in the non-medical men expressly to be made Managers.

If the three had been present at elections they could have carried no candidate, as they were from the beginning in the minority, six members, medical men, having chosen nine men not medical, and made them managers, If they had even had a majority it would have made no difference; because there were no others to choose than the non-medical men who constituted the old Board. Neither would a majority have enabled them to bring in others to make managers of; because the Board had the right by the original act of 1833 to determine the mode and manner of admitting new members, and how they shall cease to be such; and no one could be admitted unless nominated by the President or Moderator. By the operation therefore of the original act of incorporation in 1833, and the regulation made by the three medical men, the result was the same whether there was an election or not; viz. the perpetuation of the office in the hands of the Board."

The rejected report contains other accusations and complaints, which do not involve perhaps any direct criminality on the part of the Faculty, but still are of a kind to excite prejudice in the minds of the uninformed. These we shall not notice further than to say that they are fully refuted by the pamphlet, and shown to be as destitute of foundation and as little worthy of confidence as those to which we have adverted.

That the committee should have preferred the grave charges contained in this paper confessedly without "authentic information," is certainly most extraordinary, and comports badly with their claim to sincerity. And it is equally extraordinary that, after a majority of their fellow council-men had cast the thing of shreds and patches from them as though it were something unclean, they should proceed to have

it printed, and threaten the Faculty with its distribution if an attempt were made to discredit it! Would it not seem, indeed, that they feared the exposure which has followed?

Conceding, however, to the Committee the merit of sincerity, the three individuals who manifested so much interest in this report, as to lead to the supposition that they would pay a part of the expense of publication if called on, must be regarded as the true culprits. One of this worthy triumvirate, Dr. Flint, was, it will be recollected, at one time a member of the Faculty, and of course had "authentic information" on all the matters involved. No man, therefore, knew better than he did the utter falsity of many if not all of the charges contained in this report. The Board of Managers, according to this document, is illegally constituted. Yet Dr. Flint accepted office under this same body, and most reluctantly yielded up his professorial privileges to it. As a member of the Faculty he was well aware that the expenditures during the first three years far exceeded the amount of matriculation fees, and actually joined in obtaining a considerable loan to supply the deficiency. Yet he has not hesitated to give currency to a paper charging the Faculty with having appropriated to their own uses the money thus derived from matriculations. By what code of ethics this gentleman's conduct is governed, it is difficult to imagine. Surely not by that which obtains among honest and upright men. What apology he may frame for himself out of the circumstances attending his ejection from the Faculty of the Institute at a former period, we are unable to say. We can conceive of nothing, however, that would justify him in pursuing a course of conduct such as is exposed by this pamphlet, and which must be looked upon not merely as disingenuous but disreputable. C.

HOMŒOPATHY.

We have been considerably entertained by some observations on this grandest of all grand humbugs, which we find in Lea & Blanchard's Medical Intelligencer copied from the *Physiologie du Medicin* of Louis Huart. The writer after telling us that the essence of the Homœopathic doctrine is contained in the aphorism, *similia similibus curantur*, gives some instances of miraculously prompt cures, and proceeds in this wise—we translate literally:

"It is not only men that homœpathic physicians treat with the success described above. They likewise bestow their cares upon

quadrupeds generally—even upon asses. However, we do not think they do so always in pursuance of their famous maxim ***similia similibus***—that would really involve too much of Christian and homœopathic humility."

Then follows an illustration which is inimitable of its kind: a man with an enormous quantity of cravat, and proportionally small quantity of hair, is seated, with spectacles on nose—which last feature has a most inquisitive perk—peering intently on the protruded tongue, and feeling the pulse in the sinister fore-leg of a long-eared animal that lies in graceful reclination before him, with a most dolorous and ***asinine*** expression of countenance.

"Another fundamental point of the doctrine in question is, not to administer internal remedies except in infinitesimal does. This has some advantage, for without it the homœopathic practice would be much more dangerous to the public."

There is a good deal of truth, mayhap, in that last observation. After a lively and graphic description of the preparation and administration of one of these small doses, our author proceeds:

"With a lozenge [*tablette*] of jujube, the homœpathic physician would form potions to cure a whole regiment of cuirassiers affected with the whooping-cough."

But the richest conceit is the following:

"I firmly believe that the homœopathic doctrine was originally invented by a physician that had suffered a refusal at the hand of an apothecary's daughter; and from the anger of this frantic lover has arisen the system which must inevitably ruin those philanthropic establishments where they sell liquorice and senna at a profit of eight hundred per cent!"

What say you to that, reader? Is it not droll, and above all, is it not exceedingly Gallic? Apothecaries' daughters should be more circumspect.

"Hence the pharmaceutists have sworn a mortal hatred to the homœopathists, who have been obliged to prepare and vend for themselves their little packets of white powder.

Nobody would find much fault with this, if unfortunately these small, infinitely small packets of white powder did not cost as much as the great bottles of drugs under the old system!

If this is complained of, the homœopathist answers: *Every body must live!* Unfortunately this phrase is not applicable to their patients!"

Can any one tell us where to procure Huart's *Physiologie du Medicin?* It must be a rare book, racy and original. The spirit of Moliere undoubtedly dwells in the author. C.